

DIFFICILES NUGÆ:  
O R,  
O B S E R V A T I O N S  
Touching the  
T O R R I C E L L I A N E x p e r i m e n t ,  
A N D  
The various Solutions of the same,  
especially touching the Weight and  
Elasticity of the Air.

*The Second EDITION, with  
some occasional Additions.*



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THE  
PUBLISHER  
TO THE  
READER.

THE Title of this Book gives thee little encouragement to buy or to read it, and it was purposely so given. If it performs but as much as the Title promiseth, thou art not deceived; if more, thou art advantaged.

The Subject is seemingly trivial, and suitable to the Title; yet it hath exercised the Wits and Pens of many Learned men, and makes way

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to Natural Discoveries, and consequences of greater importance; at the worst it may be an innocent Diversitement, and possibly delightful to those that are pleased with Speculations and Experiments of this nature.

The Experiments therein are some new, some vulgar and common, and some borrowed from those Excellent persons, whom though the Author highly honours for their Learning and Industry, yet in some things dissent from them in their Solutions and Conclusions from those Experiments that they have most ingeniously found out and delivered.

The Author pretends not to an equality of Learning to those from whom he thus dissent, much less to Infallibility; there may possibly happen

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But he that considers the intricacy of this Inquiry, the various implications of Causes, and Effects, and Appearances that occur therein, the great difficulty of arriving to exactness in the Experiments themselves, will rectify or easily excuse such mistakes or inadvertencies, if any such occur in these Papers.

If the Inquiry hath not attained its full complement and perfection in these Papers, yet the Author hath obtained his End therein, namely, to give hints and occasions to Ingenious

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persons to make farther and better Discoveries, and to rectifie (if there be occasion) the mistakes of these or other mens Solutions ; and to give a little warning to men of Wit and Parts, not to be too hasty or positive in exterminating the Aristotelian Philosophy , and entertaining new hypotheses, 'till they have fully and maturely considered, and well looked about them. The Author's Name is not prefixed, for it would be of small advantage or use : and he is thereby the fitter to bear the correction of his Errors, and to retract them with more ease, and less observation, if any such shall occur to his discovery or notice ; and therefore hath left the Book it self, and the matters therein delivered, to bear their own burthen, and to stand or fall as they deserve.

O B S E R -



*Observations touching the Torricellian Experiment, and the Weight and Spring of Air.*

CHAP. I.

*The Introduction, containing the order of the ensuing Enquiries.*



Mong the many Experiments of latter Ages, there hath been invented that Engine that commonly goes under the name of the *Torricellian Experiment*, which is but this: A Glass-Tube of three foot or more long, closed at one end, and then filled with Mercury or Quick-silver, and then the open end stopped with the finger, and inverted into a vessel of restagnant Mercury; and when the end is sufficiently immersed, then the finger nimbly removed, so that

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no Air get in, the Mercury will subside in the Tube to the height of 29 Inches, and half an inch, or near thereabout ; but infallibly between 27 and 30 Inches, leaving the residue of the upper end of the Tube emptied of the Mercury.

This Experiment, and the solution of it, hath exercised the Tryals and Wits, and Invention of very many excellent Persons, such as were *Helmont*, *Gassendus*, *Kircherus*, *Shottus*, Dr. *Carleton*, Mr. *Hobbs*, Mr. *Sinclare*, Monsieur *Pecquett*, Monsieur *Pascall*, *Magnanus*, Mr. *Boyle*, *Linus*, *Honeratus*, *Fabri*, and divers others ; who though men of great Learning and Industry, have run into several Parties, and given Solutions, and raised Conclusions from it, extreamly contradictory the one to the other.

And although this seems but a very trifling and ludicrous Experiment, yet almost all dissenting Parties have made it to suffrage to their several preconcerned Sentiments, and Perswasions, and that in Philosophical Points of as great moment and importance per-  
chance

chance as most be to be found in natural Inquiries. Some from hence confirm themselves in their Perswasion, that there are both interspersed and coacervated Vacuities or Spaces, empty of any corporeal substance in the Universe; others again as confidently concluding the untruth of that Opinion, and that from the same Experiment. Others again from hence confirming themselves in the *Cartesian* supposition of his *Triæ Principia*, and especially of that *Materia Subtilis*, which cannot be excluded from pervading the most contumacious and solid body: Others attributing the same Power to the more subtil parts of the aerial consistence; some from hence concluding an actual pressure and gravitation of the Air upon all subiected bodies, and that thereby the Mercury is susteined by way of Pressure, and Trusion, and Counterpoys, and have substituted thereupon as an undoubted *Hypothesis*, a world of admirable consequences in natural appearances, not only in the greater World, but also in the lesser World the humane

B 2 structure.

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structure. Others again have hereupon  
maintained their opinion of Attraction  
as the necessary effect of Tension, and  
resolve this suspension of the Mercury  
unto the force of attraction and suction,  
occasioned by that Catholick Law of  
Nature for preservation of the contin-  
uity of the Universe, and all its parts,  
and the *Fuga Vacui*, and are no way satis-  
fied with that confidence that decryes  
it.

From this diversity of Judgments of  
Learned men, we may learn, first, how  
little it is in natural Effects that we re-  
ally and truly know and understand,  
when so trivial an Experiment that we  
have so easie an access unto, and handle  
with our hands, and perceive by our  
sight its motion, which yet so gravels, or  
at least divides men of great Parts, Judg-  
ment, Learning and Experience. 2. How strangely partial men are to those  
Sentiments that they have once enter-  
tained, and perchance taken much pains  
to mould and fashion, or have even  
publickly engaged unto or for; whereby  
it comes to pass that men are not wil-  
ling

ling impartially to consider what makes against their opinion, and frame a thousand imaginations to evade the strength of the opposite reasons, and to construe all appearances (as melancholick persons do the sound of Bells) to speak what they fancy; And possibly all the opposite Opinators in this business are under the like partiality and unindifferency.

There is a great odds between a Matter propounded only as an Hypothesis, and propounded as a real truth. In the former there is nothing more required than a true understanding of the *Phænomena*, and a ready Wit to contrive some Model, and to dress it up so handsomly, that it may answer the *Phænomena*, and to fit up such expedients as may meet with and stop the Leaks that otherwise would happen in the Hypothesis: And such a man tells us not so much what the truth of Nature is, as what he thinks he could have made it, if he had had the handling of it; such may be the Systemes of *Ptolomy*, *Copernicus*, and *Tycho Brahe*, which

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which cannot be possibly all **true**, though possibly they may be all false. And yet every of them is so fitted and accommodated by the Wit, Invention, and Industry of the Authors, that they solve the *Phænomena* very near equally: And thus if I remember aright *des Cartes* fairly propounds his *tria Principia*'s, and some other parts of his New Philosophy.

But to propound a thing as really a truth in Nature, is another kind of business, and requires not so much a dextrous Invention, but an impartial inspection into the things themselves, and examination how all things stand one with another, as we truely find them, and not as we mind to make them: For the Laws of Nature are stable, and settled, and regular, and not like the Laws of Men, or the fashions of our Cloaths, mutable, according to new Modes, or devices of Fancy.

Therefore the bare accommodation of Hypotheses to the Solution of the most obvious *Phænomena* is not always the measure of its truth, for that may be

be but the product of Invention and Wit. For it is apparent, that though it is impossible that all the varying Solutions of this trifling Experiment can be all true, yet they are all so dressed and pieced up, that they do solve the most obvious *Phænomena* in this Experiment, well near equally each with other.

But on the other side, if any *Phænomena* happen clearly either in this or any other instance, that do cross and thwart that which is taken up as a necessary *postulatum* in any of these Solutions; it is if not an undeniable, yet a great and possibly a clear demonstration, that the Hypothesis its self is false, and ill Founded, especially if it be the great *Basis* upon which such Hypothesis is bottomed and built.

And if the foundation of a Hypothesis, or the general *postulatum* upon which it is bottomed, be false, or only imaginary, although the Inferences and Conclusions made upon such a *Basis* be deduced with all the fineness and subtilty imaginable, and such (as were the *postulatum* it self true, upon which they are bot-

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tomed) would be as necessary and consequential, as the Demonstrations of *Euclide*, yet the whole fabrique will fail when it comes to tryal, and become like the Astrological Predictions Calculated with Art enough upon a false Ephemerides.

I that come after the Labours and Scrutinies of so many Excellent Men that have gone before me upon this Subject, cannot promise my self or others that I shall say much in this Matter that hath not been said before; and the necessity of a free Discourse requires that I should take in somewhat of other mens Labours. But I shall avoid, as much as I can, the imputation of a Plagiary, by mentioning the Authors as I have occasion: But yet, though somewhat that I shall say hath been said before, especially by *Linus*, and *Fabri*, yet somewhat will be new, although the most I shall do herein will be to make the Method and manner of Explication of it as much my own as I can, which I shall do in this order.

First, I shall explain some Termes  
which

which I shall have occasion to use, that so I may write intelligibly according to that sense I gave my words; though it may be possibly not according to the sense wherein others have used them.

Secondly, as preliminary to what I shall say touching this Experiment, I shall set down some Statical Observations, that may be useful to me upon this inquiry, not that I shall ingage my self in the whole Theory of Statical Principles and conclusions; this hath been done already by those worthy Persons that have *ex professo* handled this Subject, as *Archimedes, Steving, Mr. Boyl,* and others; but only shall glean up some general Observations therein necessary to this inquiry.

Thirdly, I shall set down what it is not, that may be supposed in the space derelicted by the Mercury, namely, it is not Nothing, nor Ayre, nor *Æther*, nor any Body that comes from without.

Fourthly, I shall set down what I think it is that possesseth the place derelict by the Mercury.

Fifthly, I shall set down what I think is

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is not the cause that suspends the Cylinder of Mercury, in the Tube to that Altitude of 29 Inches, or thereabouts; namely, not the Gravitation or pressure of the impending Ayr, nor its Elasticity.

Sixth, I shall set down what I think is the cause, that suspends the column of Mercury from subsiding in the Tube below Twenty nine Inches, or thereabouts.

Seventhly, I shall set down those objections, that seem of the greatest force against the last supposition.

Eighthly, I shall take a particular examination of some other Experiments, as touching the cohesion of Polished Marbles, and the *Magdeburgh* Hemisphere, and the rising of Water in the common Pumps.

And this Order I shall as near as I can observe in the ensuing Chapters.

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CHAP. II.

*The Explication of some Terms, that may be of use in the following Inquiry.*

**F**irst, I shall observe the difference between the specifical or intrinsique Weight of a heavy Body, and the extrinsique Weight or *pondus malis*.

For instance, a pound of Mercury, and a hundred pound of Mercury, have the same intrinsique Weight, but yet not the same extrinsique Weight: A pound of Water and a pound of Mercury have the same extrinsique Weight, for each quantity weighs but a pound, but they have not the same intrinsique Weight, for quantity for quantity, Mercury is heavier then Water.

Those Bodies are said to have the same specifical or intrinsique Weight, when the same bulk or quantity counterpoiseth equally the other; and those Bodies are said to differ in intrinsique Weight, where a greater quantity of the one is required to Equiponderate a less

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less quantity of the other ; as for instance, a Cylinder of Mercury, of an Inch long, and half an Inch Diameter, counterpoiseth a Cylinder of Water of the same Diameter and 13 Inches  $\frac{1}{2}$  or 14 Inches long, or thereabouts. But although a Body specifically or intrinsically lighter than another, may in some cases have the advantage in point of speed in motion of a Body of equal extrinsical Weight, because it takes up less room, and so meets with less resistance in the *medium*, yet upon the Scale they equally counterpoise each other.

But again, there is this difference between them ; a Body of a greater intrinsique or specifical Gravity of an equal extrinsique Weight, with a Body of less intrinsique weight, will sometimes equally press upon a Fluid Body, upon its Superficies, but will press more than the lighter, when both have the advantage to get below the Superficies ; for instance,

Take a pound of Lead, and a pound of Wood, whether lighter or heavier than a quantity of Water equal to it, lay

lay them each upon a peece of Cork that will support them in the Water; the pound of Lead, and the pound of Cork will both swim upon the Water.

But if the Cork be taken away, the Led will sink, and the Wood will swim if lighter than a like quantity of Water, and if heavier than the like quantity it will sink, but not so fast as the Led.

2. There is a difference between an intrinsique Weight, and an accidental Weight, which may be various; as for instance:

In respect of the position of the one and the other: Water and Water are both intrinsically of the same Weight, yet if Water be in a Tube, open at both ends, and be stopt with the Finger at the lower end, and then when contiguous to the Water in a Vessel, the lower end it is opened, all the Water in the Tube will subside to an equal Superficies with the Water in the Vessel, because its higher position gives it an accidental Weight more than that in the Vessel.

So again, Ayre and Ayr have the same Weight,

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Weight, though it be hardly measurable, but if Ayr be compressed as in a Wind-gun, it gains an accidental Gravity by its Compression of parts more than the common Ayr.

3. There is a difference in Terms between Gravitation, and pressure of heavy Bodies; for instance, A Tube stopt at the upper end, and driven down in a Vessel of Water forcibly, the pressure of the Water upon the Ayr in the Tube, will contract the Ayr as much as it can (suppose it an Inch) and the rest of the Ayr in the Tube will press upon the Water, because it will not yield more to the Water; this is properly Pressure in the Ayr upon the Water, and of the Water upwards upon the Ayr; yet neither are properly Gravitation, though possibly, I may in some passages use the words promiscuously.

4. There is a difference between Gravitation *ad motum*, which I call sometime sensible or perceptible Gravitation, and Gravitation *ad pondus*, which I sometimes call real or insensible, or Physical Gravitation: For instance,

If

If a cubical Vessel of Water contain 56 pound weight of Water, and a piece of Cork, or parcel of Oyl of two pound weight be placed upon the top of the Water, it will not sensibly gravitate, but will be born up by the upper Superficies of the Water, whereunto the lower Superficies of the Cork or Oyl will be contiguous, because being specifically or intrinsically lighter than Water, it is sustained by it, and makes no motion in the subjected Water.

(Fig. 1.) But yet it is plain that in concretion with the water, the whole weighs 58 pound, whereas before the water alone weighed but 56 pound; so that here is a gravitation *ad pondus* added hereunto *in concreto* to the water. But let us suppose that the Vessel *A, B, C, D*, be filled with water unto the superficies *C, D*, and the tube *E*, open at both ends be immersed into the water, and then a quantity of oyl, that is lighter specifically than water, be poured upon the superficies of the water; this will raise the water into the tube *E*, above the common superficies of the water, to such

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such a proportion of height in the Tube as will countervail the weight of a like Cylinder of Oyl, which will be somewhat near to the superficies of the Oyl, because the water at *G*, is not equally pressed with the rest of the superficies of the water by the incumbent Oyl. The disparity of the pressure causing motion or elevation of the water in the part not equally pressed ; so it is *gravitatio ad motum*, and not simply *ad pondus*. So if upon a vessel of water there be placed a Body specifically heavier than water, but not exactly commensurate to its superficies ; here is *gravitatio ad motum*, as well as *ad pondus* ; for the body sinking into the water must needs raise up as much water as the space it self takes in subsiding into the vessel of water.

And upon the same account it is, if the subsiding body be a Fluid, as *Mercury*, it will by sinking into the water drain up as much water as the room it takes, and so make the superficies higher ; so that an unequal pressure of any Fluid must necessarily make a motion upwards

upwards of the parts of it, and thereby raise the superficies by so much in bulk of water as will countervail the room that the immersed body takes up in the water. And this and the like I call sensible gravitation, or gravitation *ad motum*.

5. There is a difference between *Pondus*, and *Potentia*; and this is well enough evidenced in the instance of the Oyl and Water above given: The oyl gravitates upon all the superficies of the water, except that subjected to the orifice of the Tube *E* there is the *pondus* of the oyl and the water driving up, and sustaining the water in the Tube to that height that equiponderates a like column of oyl there is the *Potentia*.

Mr. *Sinclare* and others, that contend for the Solution of the *Torriceillian Experiment* by the Gravitation of the Ayr, apply this difference unto two instances, which possibly in the event of their Examination will appear otherwise; namely: 1. That the Ayr equally pressing the restagnated Mercury in all places thereof, but that

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which is directly under the column of  
the suspended Mercury, and thereby  
Equiponderating a column of Mercury  
only of 29 Inches  $\frac{1}{2}$  and no more, keeps  
it suspended at that height by the Equi-  
pondium of both, there is the Gravita-  
tion of the column of Mercury, which is  
the *pondus*, and the Impending column  
of Ayr, which is the *potentia sustinens*,  
that Counterpoyseth that *pondus*. I  
shall have occasion to use this difference  
of *pondus* and *potentia*, in relation to this  
Experiment also, but in a different way.

2. The other instance, to which he ap-  
plies this difference, is in all Fluids,  
whether Ayr, Water, Mercury, or any  
else: Namely, if a Body be in any  
depth of Water; suppose above it were  
20 foot of Water, and below it two,  
three, or four Foot of Water, or more,  
subjacent to the lower Base of the Body.  
The upper column of Water is sup-  
posed to press downwards, *per modum pon-  
deris*, upon the upper Superficies of the  
Body, and the lower column of Water,  
contiguous to the lower Superficies of  
the Body, is supposed to press upwards,

*per*

per modum potentiae. And he supposeth that the pressure of the *potentia* upwards, is alwayes equal to the pressing of the *pondus* downward: This is a supposition fitted principally for the accomodating of the Solution of the Cohesion of Polished Marbles, by the Gravitation and *potentia* of the Ayr, pressing upwards and downwards, and some other instances, whereunto the single Gravitation of Ayr downwards will not be sufficient: The proof whereof will be considered hereafter.

6. There is a difference between the resistence or Renitence of a Fluid, and the Gravitation or pressure thereof: If a man strikes forcibly with the flat of his hand upon the Superficies of the Water, he shall find near as much pain as if he struck upon a Board, although the Water doth not counter-move the hand, but receives and resists its sudden force; the like occurs frequently in Ayr: The fierce striking of a wand through it, bends the wand, and the fierce Collisian of the Bullet against it, is not without a resistence of the Ayr,

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though too weak to encounter it.

And this Renitence or resistance, without any counter-motion at all by the Water, is that which makes the Motion of the same heavy Body through the Water considerably flower than through the Ayr; and the same Weight of Lead or Iron, or other heavy Body in Water, to weigh less upon the Scale, than when it is in Ayr, the retardation of its Motion, and the correction of its weight, being the same effect of the same Cause; namely, the resistance of the Water, greater than in the Ayr, without relation to any counter-motion, or counter-pressure in the Water it self.

7. There is a difference between Rarefaction and Tension, and between Condensation and Compression, though the effects be much alike in both.

Rarefaction, (for instance, of the Ayr) is the Extention of its parts, commonly by heat, or what is equivalent, the fiery Partacles that it receives, whether from the Sun, the *Aether*, or common fire, or other Calefactive nature.

Ten-

Tension, is when the parts of the Ayr are distended by virtue of some force or power that layes hold of its Extremes, as a Lute-string is under a Tension to a greater length, by a considerable weight appended to it; only it hath this difference, a Lute-string or the like, is not capable of Tension in length, but it must have a contraction in thickness, but Ayr as it hath a motion every way, so it is capable of Tension every way.

The effects aswel of Tension as Rarefaction, are these, *viz.* the same substance holds thereby a greater and more extended space. 2. The Body, thus either by heat or force, Rarefied or Tensed beyond its true natural size and staple, hath a Motion of restitution, naturally contracting it self, and pressing inward. 3. The Body thus contracting it self, by the Catholick Law of Nature, to preserve the continuity of the World, layes hold upon the Bodies next adjacent, and conterminous to it, and as much as it can draws it inward.

Again, Condensation and Compression differ in this, that the former is

C 3 commonly

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They both agree in their effects.

1. That the Ayr Condensed or Compressed, takes up less room or space, than its natural and due size.
2. That they have each a Motion of restitution, by expanding it self to its just size.

And this Elatory or Spring, I allow to the Ayr; namely, of retraction, and expansion, when put out of its natural size or staple: But the imaginary prodigious Spring, attributed to the common Ayr, as its natural tendency, I take to be only invention.

It is hard to say to what proportion Ayr may be Rarefied or Tensed; some think to above 70 times its common extension, or to what degree it may be Condensed or Rarefied; some think to seven, nine, or ten times, less then its ordinary consistencies, or more: *Quod vide apud Morsen in pneumaticis, et aliis.*

8. There is a difference between the common Gravitation of a Fluid of any

any kind, as it is a heavy Body, and the appropriate Gravitation of it as it is a Fluid Body: In respect of the former, it doth as all heavy Bodies, press perpendicularly downward towards the Centre; But as it is a Fluid Body, it hath an appropriate Gravitation of its own, whereby it corrects and allayes in some sort, its common Gravitation: For instance, Water in its own consistency, hath a lateral Motion, a Motion *per declivè*, an Horizontal Motion, within the compass of its own Superficies; yea, and a Vertical Motion upward, within the compass of its own Superficies: As if a Tube full of Ayr, stopt below, be immersed into Water, and then unstopped, the Water will heave as freely upward as it would otherwise downward, till it attain its own common level or Superficies; and suitable to its Motion is its Gravitation, which is nothing else but *motus*, or *conatus ad motum*.

And besides this appropriate Gravitation of Fluids, there is a certain proper and connatural Texture in Fluids,

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and especially in Ayr, whereby the subjected parts do suspend, and hold up considerably the Superior parts from any considerable Gravitation, so that no Body, or portion of space less than the whole Base of the whole Fluid is considerably pressed upon by any such imaginary Column Commensurated to the Base of the subjected Fluid, as hath been elsewhere observed, and will be further Illustrated in some of the following Chapters.

9. There is a common and allowable difference between the pure Ayr, such as may be in the upper Region, and that which is now commonly called the Atmosphere. The former is so pure and subtil, that the greatest pretenders to the Ayrs Gravitation that I have seen, do not take upon them to determin, that it hath any Weight.

The Atmosphere, is that portion of the Ayr that is the common receptacle of Vapours, and the *Effluvia* of the Earth and Water, whereby it is less subtil than the pure Ayr.

Though some, out of the confidence  
they

they have of their attaining the just proportion of weight between Water and Ayr, and the just proportion of weight between Mercury and Water, and upon the high confidence they have of the just *Equipondium* between 29 Inches  $\frac{1}{2}$  of Mercury, and a Column of Atmosphere of the same thickness with the Column of Mercury, have undertaken, to define the just height of the Atmosphere; some determining it to be just 7000 Fathoms, some 7 Miles; yet the Doctors much differ among themselves in their account; some telling us, they take it to be 22 Miles high, others 50, others above 100.

And there must needs be an exceeding uncertainty in this way of Computing it. For first, Though the just proportion of weight between the Quicksilver and Water is easily and certainly discovered to be truly as *Mersennus*, and others, have accounted it, *viz.* that one Cubick Inch of Mercury will just counterpoise 13 Cubick Inches and  $\frac{1}{2}$  of Water; yet the proportion of weight that Ayr bears to Water, (if it have

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have any real weight at all,) is not to be exactly Calculated, whatsoever Experiments have been made thereof by *Mersennus* and others; the former telling us, that the weight of Water to Ayr, is as one to about 1300; the others telling us it is as one to 1000, or at least some small proportion less.

2. Admit the proportion were justly known, yet those that take their measure of the height of the Atmosphere, or Gravitating Ayr, (if any such be) by the supposed *Equipondium*, between 29 Inches  $\frac{1}{2}$  of the suspended Mercury; and a Column of Ayr commensurate in thickness to such a Mercurial Column, have bottomed themselves, as I think, upon a false Foundation. 3. But if it were true, as they would have it, yet even upon their own principles, it is extremely mistaken and convinced by their own suppositions and Experiments, as shall be observed in the ensuing Chapters.

And thus far to render my self intelligible in my expressions in the future Inquiry.

I shall only add some Matters that will be useful as to the point of Calculation.

The weight of Water is various, according to several places; *Stevin* tells us, that in some parts of *Holland*, a Cubique Foot of Water weighs 63 pound; as I remember, Mr. *Sinclere* Computes it to 56 pound, and consequently, a Cube of Water 6 Inches square, weighs 7 pounds; and I have by tryal found, that it weighs 7 pounds wanting two Ounces; but the Standard of 56 pound and 7 pound, ordinarily futes the proportion of weight of those two Bodies: Upon tryal, I have found the proportion very little differing, *viz.* a Cube of 6 Inches square of Water to weigh 7 pound wanting two Ounces, and consequently a Cubical Foot to weigh 55 pound 4 Ounces.

The readiest way to avoid Fractions and the long proceſs of Arithmetical Calculations, is by immersing any regular or other Solid Body into a Vessel full of Water, and to ſave that which is

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is impelled over by the immersion of the Solid Body, to dilcover the proportion of weight between such a Solid, and a Fluid of the same Moles or Dimensions.

The measure of a Cubique or Square Body, is by Multiplying the one side into the other, to find the Area of Inches; and by Multiplying the Area by the measure of Inches in profundity.

The measure of a Cylinder is by multiplying half the Diameter into half the Circumference, which yields the Area, and Multiplying the Area (of Inches) into the number of Inches, in the depth of the Cylinder: I mention these Figures, because the most ordinary and useful in Hydrostatiques.

Mercury is thirteen times and an half and somewhat more heavier than the like quantity of Water, as I have found upon tryal: If it be taken fourteen times heavier, it will not be much out of the way, and avoids Fractions. And therefore upon that computation, if a Cube of six Inches Square of Water weighs seven pound, a like

Cube

Cube of Mercury will weigh 98 pound  
Averdupoise.

The often use of Mercury even in the *Torriceilian Experiment*, but especially, where there is occasion of frequent Superfusion or Infusion of Water in the operation, will certainly much alter its Texture, Weight, and Use, as I have Experimentally found.

It is a matter of great difficulty to avoid the immission of Ayr in the *Torriceilian Experiment*, which will much disorder the operations.

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CHAP.

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## CHAP. III.

*Concerning the Gravitation of Bodyes ;  
and first, concerning the Gravitation of  
Fluids, in relation to Fluids of the same  
kind and consistence : And therein also,  
first of the Gravitation of Water upon  
Water.*

**A**LL heavy Bodyes have a Physical Gravitation belonging to them , as the natural effect of that Gravity ; for Gravitation is either Motion , or *conatus ad motum* , which is the natural effect or action of Gravity .

And yet although the parts of all continued Homogeneal heavy Bodies participate of the same common Gravity , that is common to the whole *continuum* ; Those parts are impeded in their actual or sensible Gravitation one upon another , by the support that each inferiour part gives to the superiour parts , as the upper parts of a Cube of Lead do not actually Gravitate upon the

the inferior parts, because the upper are Mechanically impeded by the lower, from their actual Gravitation upon them; yet every Atom thereof contributes to the Gravitation of the whole upon the Scale.

But if the Solid Body be not all of the same consistence, but the lower are of a lighter or more laxe Texture, or consistence than the upper, there the upper parts will not only Physically and really, but Mechanically and sensibly Gravitate upon the lower, according to the measure of their excess of weight and solidity above the lower, as if the upper part of a concrete Cube consists of Lead, and the lower parts of Clay, in process of time, at least the upper will crumble away and decay the lower, by their more prevalent Gravitation, and the imparity of the resistance and sustentation of the lower.

Touching the Gravitation of Fluids upon Fluids, of the same kind and consistence, I shall premise something; and first, concerning the Gravitation of Water upon Water, and then of Ayr upon Ayr.

It

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It seems clear, that the upper parts of Water do not actually or sensibly Gravitate or press upon the lower parts of Water; They do indeed Gravitate *ad pondus* and Physically upon the lower, so as to make the whole heavier, but they do not Gravitate *ad motum*, or sensibly: That which persuades me hereunto is Reason and Experience, or Observation.

(Fig. 2.) For Reason, I alledge, that if the upper parts, suppose the Cube of Water *C* should Gravitate sensibly upon *D*, and consequently upon *E*, in the Vessel of Water *A B*, it must either be by displacing of *D* and *E* from their position, and so *C* to subside in the place of *D*, and after in the place of *E*, or else it must be by Compressing of *D* into a shallower space; but it can be neither of these, and therefore it cannot sensibly or *ad motum*, Gravitate upon *D*, and consequently upon *E*.

As to the first Proposition, we cannot, that I know of, think of a third; It is true, it hath a Gravitation *ad pondus*, because it adds the weight of a

Cube

Cube of Water to the common weight of the Water, but as to any sensible Gravitation, or Gravitation *ad metum* it is otherwise.

As to the minor Proposition, it consists of two Parts; 1. An Exclusion of the displacing of *D*: 2. An Exclusion of its compression or contraction in height.

1. It cannot remove *D* from the place it holds, because the Cube of Water *C* is of equal weight and solidness with that of *D*; and it must be either a Body intrinsically or at least extrinsically heavier than *D*, that must make *D* give place to it.

If it be said that *D* may get out of either side, and so give way to the pressure of *C*; that cannot be, for the Collateral or Transversal Column of either side, as strongly compresseth *D* as the Cube *C*, because each side is not only pressing with its lateral pressure against the sides of *D*, But there impends upon that lateral Column a Cube or Column, of equal height with *C*, and presseth as hard upon it.

D

2. Again,

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It seems clear, that the upper parts of Water do not actually or sensibly Gravitate or press upon the lower parts of Water; They do indeed Gravitate *ad pondus* and Physically upon the lower, so as to make the whole heavier, but they do not Gravitate *ad motum*, or sensibly: That which persuades me hereunto is Reason and Experience, or Observation.

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D

2. Again,

2. Again, if *C* should actually and sensibly Gravitate upon *D*, and consequently *C* and *D* upon *E*, and so *C* should subside, that which is driven up would possess the place of *C*, and then that should subside again, and other should succeed in its place, and so the Body should be in perpetual Motion, which is not only contrary to common Sense and Experience, but to Reason also.

3. Again, (which is but a various Explication of the first instance) it is not possible for a Body of equal weight (such as *C* is to *D*) to impel or drive out of its place a Body of the same equal weight, for the resistance is as strong as the power that should move it, which necessarily must give rest; for if the Cube *D* be driven out of its place unto a higher Superficies, it must be driven up by a greater weight than it self, otherwise it will never obey it, nor yield to it.

Some therefore finding these difficulties, have resorted to the second part, namely, That the Cube *D* is comprest into a shallower consistence: But this cannot be neither.

First,

First, if it be compressed downwards; namely, between *C* and *E*, and also *E* between *D* and the Fund, then it must be comprest narrower also, because for the reason before given, the lateral pressures against the sides of *D* must be as effectual as that upon the upper Base of *D*; which would make strange work.

Secondly, Though the quality of Ayr be such, as it may be capable of Compression, yet Water is utterly uncapable of it; if we lay a weight of a hundred pound upon a Vessel of Water, indeed if it do not compress the whole Superficies of the Water entirely, it will drive it up where it doth not press; but if it strictly cover the whole Superficies of the Water, it will never press it shallower.

Thirdly, It is more evident to Experience, that the lower parts of the Water are not pressed by the upper: For first, If it should be so, a Tube of Water, suppose ten Foot long, filled and closely stopped at both ends, wou'd break the Glass, if it lay Horizontally,

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or if a reclined Tube were exactly filled, yet being set up perpendicularly, it would leave a vacancy, which it will never do, notwithstanding the pretended Accession of the Gravitation of Ayr to boot. Again secondly, Let any man try it in a Tube of Water of considerable length, and put in a little button of Wax, inclosing a peice of Lead to make it sink, it will sink to the bottom of the Water, rather with some little degrees of Acceleration, the lower it descends; which could never be, if the Water were more compact and prest below than above.

As to Experiments and Observations, it is agreed by all persons, that have treated of Hydrostatiques, that I know, only one excepted, That if a Solid Body, suppose of Wood, of equal weight with the like quantity of Water, be placed in Water, it will rest in all Positions under the Superficies of the Water, which could not possibly be if the Water towards the bottom were more compressed than it is towards the Superficies: This is agreed by Archi-

medes, ap  
em

medes, the Master of Hydrostatiques, in his Book, *De Insidentibus Humido*; by Stevin, in his *Hydrostatical Elements*, Prop. 4. By Mr. wallis, Mr. Boyl, and others.

Only a late Author, in his *Hydrostatical Theorems*, tells us, that such a Solid will emerge so, that its upper Superficies will be contiguous to the Superficies of the Water; And this he concludes, not so much upon his Observation or Experience, for ought I can find, as upon this very supposition, that the Water below is more compressed than that above.

It is a difficult matter to find any Solid by its own specifical uniform weight, to be just equal to the weight of a bulk of Water of an equal dimension to it, without application of some subsidiary supplyes to equal it, which possibly may make the Experiment it self uneffectual; yet upon the best tryal I could make by such subsidiary applications, I rather find such an Equiponderating Solid rather to be apt to subside to the bottom, than emerge to the top. D 3 2.

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2. Again, the next Experiment which I shall mention, shall be that of the same Author, in his seventeenth Experiment, wherein, if I understand him aright, he contradicts what he had delivered in his fifteenth Theorem: *viz.* If a Glass bubble with a long stem turned up, heated, and then the stem Hermetically sealed and depressed into the Water, by a convenient weight of Lead, and the Bubble tyed with a string to the Beam of a Ballance, with so much weight as might counterpoise the Bubble, then the stem broken with a Forcept, whereby the Water entred and half filled the Bubble, the Bubble subsided and required an addition of a farther counterpoise of 4 Drams and 38 Grains, to reduce it to an Equilibrium, then the Bubble taken out, and the Water driven out of it, weighed 4 Drams and 30 Grains: This instance being given by a worthy Person, to prove, that Water weighs in Water: But the Author, in answer thereunto, clearly evinceth that the Water in the Bubble is supported by the subjacent Water, and concludes evidently thereupon, that

Water

Water weighs not in Water.

3. Again, if Water should sensibly Gravitate upon Water, it must as sensibly Gravitate upon any Body subjected in it, especially if it be at the Base or fund of the Water: But the Experience of divers, of the agility of Fish of a great breadth, lying at the bottom of the Water, and infinite more, do sufficiently evince, that the weight of the superior parts of Water Gravitate not upon the inferior parts, with any sensible pressure, for if it should, it must of necessity Gravitate upon the interjacent Water, lying between the uppermost Cube and the Body; and if it should do so, it must considerably Gravitate upon the subjacent Body.

But this *Non-gravitation of Water upon Water, or upon subjected Bodies, and the reasons thereof*, I have elsewhere examined.

(Fig. 3.) An excellent Person, in the first, second, and last Chapters of *Hydrostatical Paradoxes*, hath endeavoured to prove the actual Gravitation of the

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Superior parts of Water upon the inferior, by three instances, *viz.* If in the third Figure, the Vessel *AB* be filled with Water unto the Superficies *c d*, and then the Tube be by suetion filled with Oyl of Turpentine (a Body lighter than so much Water) be stopped nimblly at the top, whereby the Oyl remains suspended, and then the lower open end be immersed in the Vessel of Water unto any depth, suppose *H*, and then the Finger be removed from *G*, the Superficies of the Tube, the Oyl will be kept up to such a height as may counterpoise the like Column of Water in the Vessel ; and if it be immersed lower, the Oyl will rise in the Cube considerably higher, and if it be lifted up higher, nearer to the Superficies of the Water, the Oyl will subside lower, and discharge part of it self into the Water, because then the like Column of Water in the Vessel grows shorter, and hath a less counterpoise to the Column of Oyl in the Tube, which he concludes to be a concludent evidence of the various degrees of Gravitation

of

of the Water upon the various imaginary Superficies thereof. The second instance is like it, namely, That if the same Vessel were filled with Oyl to the Superficies *C D*, and the Tube *g* filled three or four Inches with Water by suction, and the upper end *g* nimbly stopt with the Finger, and then the lower end *H*immerfed in the Oyl, the deeper it is immersed, the higher the Superficies of the Water in the Tube will remain sustained, and the nearer it is brought to the Superficies, the lower it will subside, by reason of the various degrees of Gravitation of the Oyl in several imaginary Superficies; though at the highest elevation of the Tube of Oyl, in the first instance, the Oyl, in respect of its less Gravity than Water, will sit somewhat higher in the Tube than the Superficies of the Water; and the Water in the Tube, in the second instance, will sit lower than the Superficies of the Oyl, because exceeding it, quantity for quantity in specifical Gravity. 3. The third is that of the value, which deeply immersed in

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**in Water, the cover of the value will  
be sustained by the Gravitation of the  
Water, with a considerable weight ap-  
pended to it.**

But it seems to me, that these Expe-  
riments concludes nothing to this bu-  
siness; but only evidence a resistence  
of the Water to that Body which will  
raise its Superficies. For it is appa-  
rent, that there is required a pressure,  
or force, to raise the Body of Water  
to a higher Superficies, exceeding the  
weight of so much Water as is so im-  
pelled higher than before, or at least  
equal to it.

Now the pressing down of the Tube  
of Oyl into the Water, or the Tube of  
Water into the Oyl, raiseth the Super-  
ficies of the Water or Oyl so much as  
the Tube is impressed into the Water  
namely, a quantity of Oyl or Water  
in the Vessel, equal to the Body impre-  
ssed and immersed in it, is raised there-  
by, and put out of its former place  
which it contends against, by a Re-  
sistence resistence, or contrary pressure  
and as the less of the Tube is immersed

th

the less Liquor in the Vessel is moved up; so the more of that Body is immersed, the more is pressed up, and consequently, the greater pressure is made upon the Water, the lower it is immerled; because it takes up more room in the Water, and a greater force is thereby exercised, to the elevation of a greater quantity of Water; and that Water, the more it is, hath the more accidental weight, or rather, resistence, against that force which thus raiseth it up above its common superficies, which before it obtained.

And that this is the true reason of it, and not the various Gravitations of the superior parts upon the inferior, seems evident by this one instance, which because it will be useful hereafter, upon other occasions, I shall here insert more largely, though the thing be commonly known.

I filled a Tube of Lead of six Inches Diameter, and four Foot deep, and close at the bottom with Water; And took a Porringer of Pewter, five Inches Diameter, and about two Inches deep,  
and

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and filled it with leaden Shot, and pou-  
red Water into it, to fill up the chinks,  
and making four holes, I suspended  
it with a Packthrid, like the Dish of a  
pair of Scales, and exactly weighing  
it, and the Shot, Thred, and Water, it  
weighed all exactly 78 Ounces, wan-  
ting  $\frac{1}{4}$  of an Ounce; then suspending  
the Dish with a Packthrid, to the end  
of the beam, I set it down 40 Inches  
deep into the Water, below its Super-  
ficies, and it lost its weight, by the  
resistance and crassitude of the *medium*,  
*viz.* the Water 9 Ounces  $\frac{1}{2}$  of its  
weight in Ayr; and so weighed 68  
Ounces, and near about  $\frac{1}{2}$  of an Ounce;  
and just the same weight it held, when  
raised 15 Inches, when raised 28 Inches,  
though it was then within 12 Inches  
of the Superficies; but at last, when I  
raised it 12 Inches higher, so that  
the Superficies of the weight was  
equal with it, and yet it lost not above  
 $\frac{1}{4}$  of an Ounce; which I decrement,  
was, as I think, because possibly the  
four strings that sustained it were not  
now under Water.

What

What then is the reason why, when the Tube of Oyl or Water is sunk lower, the pressure of the Water is unequal? and why in this instance, the weight of the Dish is no more charged with the weight of the Water, at 40 Inches, than when just even with the Superficies of the Water: I say it is not the Gravitation of the superior parts of the Water upon the inferior, for then my weight of 5 Inches Diameter could never keep the same weight at 40 Inches depth of Water, at 12 Inches depth, and just at the Superficies; but the true reason is, because in the weight of the Porringer and Shot, being the same bulk at 40 Inches deep, as at the very Superficies, drives up no more Water out of its place at one station, than another; namely, a bulk commensurate to the bulk of the Porringer, Shot, and included Water, which is the same, both at the fund of the Tube, and when its Superficies is contiguous to the Superficies of the Water: But in the instances of the Tube, if it were suppressed an Inch Dia-

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Diameter, when it is immerfed an Inch its *potentia*, lifts up but a part of Water commensurate to that Inch ; and therefore the refiſtance, or Renitence between the Tube, and the Water, is but little ; but when it is immerfed ten Inches, there is ten times more Water driven up, and therefore the Renitence is the greater, and impels the Liquor included in the Tube the higher ; But this concerns nothing the weight of Water in its quiet confiſtence : His other Experiments, in order to the proof of the Gravitation of Water upon Water, I shall consider in the fifth Chapter.

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CHAP.

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## CHAP. IV.

*Concerning the Gravitation of the Ayr upon Ayr, or any subjected Bodies in it.*

**T**HE Question in hand concernes not Ayr put out of its ordinary texture, but touching the free Ayr, wherein Men and Animals live and breath.

And now to state this Question a-right, we must consider, 1. What the Question is not, and 2. What the Question is.

1. The Question is not whether Ayr compressed and thrust together, hath a weight in it that may put it into a considerable Gravitation upon other Ayr, either not compressed at all, or less compressed; for it seems very probable by the Experiments of these Excellent Men, *Mersennus*, *Shottus*, Mr. *Boyl*, and others, that have set themselves unto tryals, in order to this inquiry,

quiry, that such a Gravitation may and doth happen by the compression of Ayr in *Æolipiles* and Wind-guns, that such Ayr may have a pretty evident preponderation to so much free and common Ayr.

2. Neither is it the Question, whether there may not be, or are not interspersions of Vapours *Effluvia*, and *Effluvia*, or other *Moleculæ* even in the free and common Ayr, that considered in themselves, have a preponderation even to the Ayr it self; for although the strict intermingling, interweaving, and contiguation of some Vapours and *Effluvia*, grosser and weightyer than the Ayr it self, may enable the Ayr to sustain and bear up many of these grosser Bodies, (as Water oftentimes sustains minute Bodies, or Gravels of Sand specifically heavier than the Water) yet we every day see, that if these portions of interspersed Vapours or *Effluvia*, grow too heavy for the Ayr to sustain them, they are precipitated out of it, and discharge themselves upon the Earth, as we see in Hail, Snow, Rain, (t  
and

and Mist. 3. Neither is the Question, whether a portion of the free Ayr, separated and disjoyned from all communication with the foreign Ayr, hath any Gravitation or no, for it seems to me, that it may have, by such a separation an actual Gravitation, which is not by reason that it acquires any new quality or accession of weight, than it had before, but by reason of such separation it doth *Removere prohibens* of its actual Gravitation; namely, the contignation that it hath with the common Ayr, into and with which it was in continuity, it was interwoven, and that little inconsiderable Gravitation that it naturally and simply confidered hath, is overmatched and broken by those interveining Filaments of the rest of the Ayr, with which it is in continuity.

And therefore I do think, that if Ayr of the same consistence with the Ayr we live in, were freely let into a Bladder, which we will *per impossibile* suppose to be destitute of all weight, such a Bladder of Ayr, thus filled, (though without the help of *inflation*,  
E by

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by our *longes*, which must needs carry  
with it some fuliginous Ingredients;) I  
say, such a Bladder of Ayr would sub-  
side in the same common Ayr out of  
which it was taken, and would Gravi-  
tate upon the Ballance, more than the  
like quantity of free and open Ayr  
incumbent upon the other Ballance.

Nay farther I dare adventure to say,  
That if we could suppose a Tube, open  
at both ends, so long, as to reach from  
the top of the Atmosphere, unto this  
Earth, such an included portion of Ayr,  
severed from communion with the  
Atmosphere, would have some Gravita-  
tion more considerable perchance,  
than we have ever felt from the open  
Ayr.

For I do much attribute the exclu-  
sion of all sensible Gravitation of the  
free Ayr, to that mutual interweaving  
of the Filaments of Ayr one into an-  
other, like a vast Net, with small Mashes  
or *interstitia*, filled gradually with parts  
more and more subtil; and this contig-  
nation sustains and keeps the parts of  
it from that separation, that otherwise  
the

the interposition of a groffer body would intercept: And although Water, in respect of its weight and texture, be more subject to disjoyn from the rest of its body, than Ayr is, we shall find in the next Chapter somewhat Analogous to this, even in a consistent Body of Water.

4. Neither is it the Question, whether even the free Ayr it self may have some intrinsique Gravity, though admirably little and inconsiderable; for if we do suppose the compression of divers Particles of Ayr may render that compressed Body of Ayr sensibly heavy, we may not wholly exclude those Particles from all kind of Gravity before compression, for no weighty Body can arise from the coalition of such parts as had no manner of Gravity before.

5. Nay, yet farther, the Question is not, whether as the free Ayr hath some intrinsique Gravitation; so neither is it the Question, whether this free Ayr hath not some, though very inconsiderable measure of actual Gravitation;

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it is not impossible but it may have some, though scarce perceptible to sense, notwithstanding all the inter-spersions and mixtures of Vapours and *Effluvia* from the Earth and Water.

But the Question is, whether the free and common Ayr, wherein we live, which is commonly called Atmosphere, extending upwards about seven Miles or more, hath any sensible actual Gravitation upon the lower World. 2. Admit it have; whether any determinate portion of that Atmosphere, as a Column or inverted Cone, for the purpose, of six Inches square at the Base, hath any actual Gravitation upon the subjected Base, upon which it is supposed to rest. 3. Admit it hath, whether that Gravitation be of any considerable, and of what moment.

Before I come to discuss the Questions themselves, it will be necessary to repeat somewhat that I have before said, touching the proportion of Gravitation, that is allotted to a portion of, for instance of six Inches square of the Atmosphere, when it perteingeth near to the Superficies of the Earth. A

**A Cube of Water, six Inches square, contains 216 square Inches, and upon the exact Calculation of those that have tryed it, weighs just seven Pounds, and a Cube of twelve Inches square of Water weighs 56 Pounds.**

A Cube of Quicksilver weighs fourteen times as much as the like quantity of Water, though *Mersennus*, and some others, nearer to the truth, compute it to be thirteen and  $\frac{1}{2}$ ; but I will make my Calculation by fourteen, to avoid Fractions.

The consequence whereof will be, that a Cube of six Inches square of Mercury, will weigh 98 Pounds, which is fourteen times as much as the like Cube of Water.

The consequence whereof is, that  
fourteen such Tubes of Mercury weighs  
1372 pound.

In the *Torrilellian* Experiment, the Mercury is sustained to the height of 29 Inches  $\frac{1}{2}$ ; to avoid Fractions, let us reckon it 30 Inches, be the Column never so large.

If the Column of Mercury be six

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Inches square at the Base, then there will be five such square Cubes of Mercury in that Column will weigh 490 Pounds: According to the late Philosophy, this Column of Mercury is counterpoised with a Column of the Atmosphere of six Inches square, and extending from the restagnant Mercury to the upper surface of the Atmosphere.

And consequently, this Column of Ayr or Atmosphere, doth actually gravitate with a weight of 490 Pounds, upon the restagnant Mercury, and really and actually weighs as much as that comes to; and consequently, when I hold a Trencher in my hand of six Inches square, there is incumbent upon it 490 Pound weight of Ayr, though there be found a help in that case to support it, by the recouling Column of Ayr, commensurate to it; whereof in its due place.

And this is the supposition I contend against in this paper, *viz.* that 1. The whole Body of the Atmosphere hath no considerable Gravitation either upon its own

own parts, or upon the subjected Body of the Terrestrial Globe, much less such a prodigious Gravitation as is here supposed. 2. If it had, yet any given portion or Column of the Atmosphere, hath no such Gravitation.

Touching the Gravitation of the whole Atmosphere in general, upon the Terrestrial Globe entirely, I shall not say much, because it is not capable of Experiment, only my reason and sense suggests it to me, that it is neither evident nor likely to be of so vast a Gravitation as the late Philosophers affirm.

First, I confess I am none of those Adepts in Philosophy, that can tell us how to solve all the effects in Nature, without recourse to the infinite Wisdom, Power, and Goodness of the Glorious God, who certainly knew better how to frame the World, and fix the Laws of Nature, than the wisest of Men or Angels. I must confess, I know not how to resolve the reason of the Motion of weighty Bodies downward nor why or how either the Sun holds his regular Motion, if the Hypothesis

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of *Ptolemy* be true, or why the Earth, as other Planetary Bodies, holds its regular Motion, if the Systeme of *Copernicus*, and *Galilaeus*, be true, and a thousand such instances; but into the primitive disposition and ordination of the Most Wise, Powerful, and Soveraign Lord and Maker of the World, who hath fixed the Order and Law of Nature, by his Soveraign *Fiat*, with the most exact accommodation of it to the concerns of the Universe, and all the parts thereof, and with most incomparable Congruity. This Soveraign Architect, that hath made all the parts of this *Mundus aspectabilis* in Order, Weight, and Measure, and designed the Earth and Ayr, for the habitation and use of Men, and all other breathing Animals, and likewise for Vegetables, hath so ordered and digested the texture of the Ayr, that the inferior part is more groſs than the superior, and fitted for nourishment and respiration; the upper parts more subtil, and pure, and light, and destined to other uses, hath ballanced the Ayr, so that the more light

light and subtil parts are inclinable per chance, more to ascend ; and the lower parts gradually more laden with Particles of a grosser allay , and so possibly thereby inclinable to subside ; and he hath connected the more subtile , and the more Feculent parts thereof, so together, that they hold an *Equilibrium* as a state and posture more serviceable to the ends and uses he designed for it , and for which they serve : The proceeding of this Soveraign Architect in the Frame of this great Building of the Universe , not being like to the Architecture of men, who begin at the bottom ; but he began at the Roof , and Builded downwards , and in that process, suspended the inferior parts of the World upon the superior. But this kind of reasoning, I know, is not grateful to the palate of the present Philosophers ; I therefore proceed.

Secondly , Therefore I say , that the common appearances of the World, so far as they are obvious to our common sences, contradicts this immense, nay, or any considerable Gravitation of the

Atmo-

58 Observations touching the Atmosphere upon subjected Bodies ; It is hardly conceivable, how Birds could raise themselves upon their wings , and keep themselves in free Ayr , if a burthen perchance of above 1000 pound weight should lie upon their Wings and Bodies ; it were not possible for me to breath, to walk , to stand upon my Feet , if at every time I open my Mouth , a Column of Ayr of three hundred pounds weight were pressing into it, it would tare open the valves of my *Larinx*, and blow up my Stomach and Intrals, like a Bladder, and break my Ribs, if it had an admission ; and if I had not the circumjacent pressure of the Ayr , would press me to death These and the like instances render the prodigious Gravitation of the Ayr incredible to us vulgar Souls , that are apt to credit our fenses.

But I very well know , that these will be said to be vulgar common places , and that they have many handsome Solutions of these common difficulties.

And it is true , I very well know the

they have colourable Solutions for those sensible occurrences, as the supposed counterpoise of included Ayr, and of the recouling Columns of Ayr; which I shall in due time consider.

But I do reply, that though these common instances perchance amount not to demonstrations of the untruth of the Hypothesis, yet they do require a very great and very clear evidence to encounter even those vulgar experiences; and therefore, if any Solutions offred of them, are not perfectly concludent, but strained, and the instances they give in their favour, are capable of any other Solution, than this which is so visibly and apparently contrary to our sense; such Solutions and such instances are too weak and impotent to build such uncouth conclusions upon.

Thirdly, Therefore I say, there is not any one instance or experiment, that ever I yet heard of, for the evincing of this imaginary Gravitation of the free Ayr, but is readily capable of a more suitable Solution more adequately fitted

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fitted to the *Phænomenon*, and less incumbered with difficulties than this new Solution, by the Gravitation and Elatory of the Ayr. I shall refer myself herein to Mr. *Linus* his *Tract De Insperabilitate Corporum*: And *Honestus Fabri*, in the 6<sup>th</sup> of his late *Philosophical Conferences*, wherein all the instances given from the *Magdeburg Engin*, and the *English Ayr Pump*, and particularly the *Torriceillian Experiment*, are sufficiently and much more satisfactory and naturally solved, without recourse to this imaginary Weight or Spring of Ayr.

And there are above an hundred Experiments of this kind and nature, which can never by this solution be explicated (without intolerable torture of them) some instances whereof we shall in due time remember.

But omitting more that may be replied in general to the common Gravitation of the Ayr; I shall more particularly and closely apply my self to the second.

3. Therefore, admitting it were possible

possible to suppose the whole Body of the Atmosphere might Gravitate upon the Earth in general, yet no one particular portion of the free and common Ayr, impending upon any one determinate Superficies, can at all or at least can sensibly or considerably Gravitate upon any determinate *Basis*.

And the reason seems to me to be this, because although we should admit a considerable Gravity of a portion of Ayr taken, divided and separated as one intire separate Grofs Body ( as in a Bladder, a Glafs Bubble, or the like ) so that it partakes of the common quality of heavy Bodies, yet there is a peculiar pressure or Gravitation belonging to this subtile Fluid Body, which hath its lines of direction every way within the compafs of its own extream Superficies, whereby that perpendicular Gravitation, which is common to all Bodies, is corrected, abated, and in a great measure suspended. 2. Because that if there were no such allay given to its common perpendicular Gravitation, by its proper Motion or Gravitation of its

own;

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own ; yet the Ayr being one continued Body, and so interwoven and mortised as it were , one part in another , the other parts of the Ayr that are contiguous every where to the Earth , do sustain and bear it up , like the sides of an Arch , from any sensible pressure or Gravitation upon any determinate or particular Body , that is within the compass of its pressure ; as I have elsewhere inforced the Argument in relation to the Gravitation of Water upon subjected Bodies , narrower than the common Base that supports such Water.

And although the Ayr hath an intermingling with it of Vapours and Terrestrial and Aqueous *Emissiva* heavier than it self ; yet they are so interwoven in the very Webb and texture of the Ayr , that it supports many of them , and those that are too heavy for it , or much disunited and separate from it , are precipitated upon the Superficies of the Earth , and the Ayr discharged of them , as in Rain , Snow , Hail , sub siding Gravel Sand , &c.

And although the instance that

am now giving, is more proper for the next Section, yet it is in some kind suitable for the explication of what I intend.

Of all hands it is agreed, that the Water, though a Fluid Body, is, some say, thirteen hundred times heavier than the like portion of Ayr; others, that it is a thousand times heavier; they that speak least, say it is above nine hundred times heavier than Ayr: And it is obvious to any that attends it, that there is not the same strict Cohesion of one part of Water with another, as is of one part of Ayr with another, unless divided by a more firm and stable Body then it self: And therefore there is far more reason, that one part or Column of Water in its consistency should Gravitate upon a subjected Body, than there is, that a Column of Ayr should in the free and open Ayr, Gravitate upon a body subjected to it; And yet it will be found, that in *libera aqua*, that Gravitation that it exerciseth upon a Body subjected to it, and capable of it, is not above one half so much

as

as the weight of such a Column of Water, divided and separated from the common consistent Water.

It is true, I had not so dextrous a Messenger, to make the Experiment in the bottom of the Sea, or <sup>20</sup> Fathom within Water, as *Dromo* is Poetically related to have done, though I believe he never made that tryal. But I will give an account of what I have tryed and found.

(Fig. 4) I took a Glass Siphon of a quarter of an Inch Diameter, the longer legg 32 Inches, the shorter 8 Inches long, open at both ends.

I filled the short legg with Mercury which, accordingly as it must rise to 8 Inches high in the longer legg, and then stopping the shorter legg with my Finger to avoid any violent pressure by the fall of the Water upon the Mercury, I then filled the longer legg to the top with Water, and gently removing my Finger from the shorter legg, to avoid too violent an Exsilio[n] of the Mercury, I found the Mercury to subside in the longer legg two Inches and a quarter, and

and as much driven out of the shorter legg, by the accession of the weight of 24 Inches of Water in the longer legg, (which I confess is somewhat less than the proportion of weight between Water and Mercury; wherein, according to an exact calculation, 28 inches of Water counterpoiseth 2 Inches of Mercury. But then emptying the Water and Mercury out of the Tube, I again filled up the Tube with Mercury, to 8 Inches in both leggs, namely to the top of the short legg.

And then filling a glass Tube, of about three Inches diameter, and 32 Inches long, with Water, I immerfed the Syphon with the Quick-silver to the bottom of the greater Tube full of Water. And although the column of Water in the great Tube, impending upon the orifice of the shorter legg, was full 24 Inches, yet it drew down the Mercury in the short legg, and raised it in the longer legg, empty of Water, only one Inch, and no more; whereby it did rise in the longer legg to 9 Inches, and subsided in the shorter to 7 Inches,

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as I could easily perceive through the great Tube, by a Scale of Inches, and quarters, fitted to each legg of the Syphon.

I then tried it doubly, *viz.* pouring in the Water into the longer legg of the Syphon, whereby the subjacent Mercury subsided in the longer legg two Inches, and as much thereof driven out of the shorter legg, and then immersed the Syphon into a glas-vessel, 32 Inches deep, filled with Water; the Mercury thereupon subsided in the short legg somewhat neer an Inch, and no more, and accordingly impelled up the Mercury and incumbent Water in the longer legg neer an Inch: whereby it appears, that there was no *Æquipondium* between the pillar of Water included in the Tube, and the imaginary pillar of Water in the open vessel.

And in this experiment I must remember, that the orifice of the shorter legg of the Tube, and the legg it self, was of somewhat a larger diameter than the longer legg; and that might give some advantage to the pressure of the Water in the vessel upon the shorter legg

legg. But notwithstanding that advantage, the gravitation of the external Water, or any imaginary column thereof was not half so much as the gravitation of the Cylinder of Water included in the Tube; which is sufficient to convince the mistake of those, that assign an equal pressure to an imaginary column of free Air or Water, with an equal column of Air or Water that entirely presseth upon the Mercury, and hath no other circumjacent base to lean upon.

And it is to be noted, that although, as I have said, a Cylinder of two Inches of Mercury doth really counterpoise a Cylinder of 28 Inches of Water, of the same diameter, yet in this instance of the Syphon, 24 Inches of Water drew down two Inches of Mercury, and raised as much out of the shorter legg. And the reason seems to be, because the Mercury being *in aequilibrio*, was more capable of a sensible pressure by a less quantity of Water in this *libra naturalis*, than it would be, if both were weighed in their gross consistence in an artificial Scale.

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And the reasons why the pressure through the Tube, is stronger than the pressure by the open vessel of Water, are these: 1. Because in the Tube the Water included had no communion with any open or free Water, nor had any sustentation thereby, but in the common Water there is a communication and mutual sustentation of one part by another. 2. And principally because when in the Tube the Water had no other Base but the Mercury, to which it was commensurate, and therefore singly, and entirely, and adequately pressed it. But the Water in the vessel had another Base circumjacent to it, namely the bottom of the vessel, from which it was built like an Arch over the short legg of the Mercury, whereby the weight, pressing upon the Mercury, was broken, abated, and intercepted. For Water will find the lowest Base for its rest.

And surely if this be true in Water, as upon more than one tryal I found it, it will be much more so in the open Air, which will much disorder the whole hy-

*potthesis*

*pothesis* of the Gravitation of the Air, and those very subtle and fine Conclusions that are spun out of it, and built upon it.

But I would not be mistaken, as if I meant that the Column of 24 Inches of free Water did gravitate upon the orifice of the shorter legg of the Syphon, to the weight of neer an Inch of Mercury, for That is wholly contrary to my Supposition: but only I hereby shew the great disproportion of the appearing Gravitation of Water, where it hath no lower nor other Base upon which it leans, but only the body of the Mercury, and where it hath another or lower Base upon which it leans.

For in this instance of the Syphon, there is another reason of the Gravitation of the free Water upon the cavity of the shorter legg of the Syphon: for here the pressing or keeping down of the body of the Syphon into the free Water, displaceth and raiseth up a portion of Water answerable to the bulk of the whole convex Superficies of the Syphon, which is far greater than its

70 Observations touching the cavity, and that Water so displaced doth indeed press upon the Tube that displaceth it; and because it can find no part yeilding to its pressure but the orifice of the shorter Tube, it presseth there in such proportion, as the whole bulk of the Water, so lifted up by the immersion of the Syphon, bears to about an Inch of included Mercury, and 24 Inches of the Water included in the Tube. But of this more distinctly and exactly, when I come to examin the reaſon of the Valve.

2. But having thus offered my reaſons againſt this prodigious Gravitation of the imaginary Aery Cylinders, I ſhall proceed to ſome plain and homely Experiments and Reaſons, which ſeem to me very much to encounter the Suppoſition.

I ſhall begin with that whereunto I am ſure they are furnished with an Anſwer, but ſuch an Anſwer as will give me the opportunity of a Reply, which poſſibly may be of more moment, than if the Experiment it ſelf were without an Anſwer.

Take a pair of Scales, the one Dish of 11 Inche

inches of diameter, the other of 3 Inches diameter, yet of that thickness, that it may be just of the same weight with the other, each dish weighing for the purpose three Pounds. These two Dishes will yet exactly equiponderate, yet the one sustains a Column of Air of above twelve times the weight of the other, *ea and according to the proportion above computed*, possibly the greater Column impending upon the greater Dish, may weigh above 4000 weight.

But there is a common Answer singularly fitted to this difficulty. For it is said, there is a Column of Air below each Dish, that hath a Superficies commensurate to the lower Base of each Dish, which though it be but 3 or 2 or foot, nay but an Inch above the Earth, bears up each Dish as strongly as the Column of Air of 7 miles high bears them down, and so there is an *equiponderum* between the *pondus deprimens* upon the upper side of the Scale, and the *patientia sustinens* of the lower side, and the Pillar of a Foot of length, between the Earth and the greater Scale, is of a

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Foot diameter, and sustains it as forcibly as the *pondus deprimens* of a Foot diameter depresseth it, and so it is supported by an equal force impelling it upward, as it is depressed by the weight of Air incumbent upon it. And the case is the same with the lesser Scale, and so the greater Scale, though it hath a greater weight than the lesser impendent upon it; so it is recompensed with a more forcible power than the less hath to sustain it, and so there is an *equipondium*.

But although this seems marvellous, that the *potentia* of a Pillar of Air from the Earth to the Base of the Scale should counterpoise so vast a weight, as what is incumbent upon it, yet to serve the turn, and to give a Solution by the weight and power of the Air to maintain the cohesion of two polished Marbles (*de quo infra*) this reason is given, that as there is a vast weight of Atmosphere upon the Scale, so there is the same Gravitation of Air round about it all, but what is just under the Scale; and this impending Air round about the sides of the Scale, as it is so much, so it is

is somewhat weightier than that which impends upon the Scale; for this reaches only to the Scale, and that reacheth somewhat farther in length, even by so much as the distance is between the Earth and the Scale; and this doth so gird and help in that short Column of Air subjacent to the Scale, that it is as firm a Pedestal or *potentia* to sustain the Scale, and to press it up by the auxiliary Columns of Air, that supply and support it, as the impending Column is to depress it, and so between both there is an *equipondium*. So that in all cases, as well in Air as Water, the subjected Column sustaining and impelling upwards, and the *potentia* thereof is of equal force to the *pondus* of the superior Column, depressing the upper Superficies of the intermediate Body.

And the same they suppose in Water, the instances whereof is the Valve, and some other instances; but of these in the former Chapter.

And now upon this Supposition of the *potentia sustinens* of the recoiling Pillar, as I may call it, sustaining and impelling

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peeling up the lower Superficies of the body, they have raised most marvellous Paradoxes and Consequences, namely, that if a Milstone were by some art suspended in the Air, if it were possible that the Pillar of Air incumbent upon it were either wholly taken away, or deprived of all Gravitation, this Milstone would be sustained by the subjected Column of Air, and possibly carried up much higher; yea, and if a long Column of Brads were let down deep enough into the Ocean, they assign a depth, at which the Column of Brads would sink no lower, but be sustained by that marvellous *potentia* of the recoiling or subjected Column of Water, things that I confess are wittily deduced, and consequential enough upon their *postulata* admitted, but such as are warily enough propounded, because impossible to be tried.

But in answer to this Question likewise, I say this will be found untrue, and therefore although the Consequences are Logically enough inferred upon such premises; yet the thing proving false

false in the event, the Premises, Principles, and *Postulata* themselves must needs be fictitious and vain.

Suppose therefore two small Cylinders of Brads, each of 4l weight, but the one of two Inches diameter, the other of one Inch diameter, and therefore about double the length of the other, were weighed in a vessel of Water by a pair of Scales, with the Superficies of each just even with the Superficies of the Water in the vessel, they will each lose a just equal portion of their weight by the thickness of the *medium*, and will still hold their *equipondium*, yet the imaginary column of Air impending upon the two-Inches Cylinder above treble the weight to that impending upon the one Inch diameter, and here is no recoiling Column of Air to sustain them.

But I easily foresee the Answer to this Instance, and therefore I gave it to let in what I have to say; for it will be said, that the Gravitation of the Air upon the residue of the Water, gives a greater concrete weight to the Water, by so much as the Column of Air, commensurate

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furate to the Superficies of the Water, adds to it, and so both Air and Water make one common pressure, and consequently the recoiling or sustaining Column of Water, hath as great a *potentia sustinens* as the weight of both put together can contribute. But to avoid this *Effugium*, first I took two Brass Cylinders, weighing each four pound, but one of double the others diameter, I took then two Laton-Tubes of 6 Inches diameter, and about two foot in length, closed at both ends, only in the centre of the close Cover of the one I caused a hole to be made, commensurate to the Superficies of the Cylinder of Brass of 2 inches diam. & a like hole in the other, commensurate to the cylinder of Brass, of 1 Inch diameter, so that they might just play upon them without any considerable distance between the weights, and the concavity of the holes. I then filled the vessels brim full of water, & weighed the Cylinders with their Superficies just equal with the Water and Cover. Here could be no Gravitation of the Air upon any part of the Water, but only upon the

the Weights, so that the Column of Air, pressing upon the greater Weight, being, as is supposed, above treble to what presseth upon the less, must needs drive it down, because the recyling Pillar of Water, could not counter-vail such a disparity of *pondus* of Air.

And yet I found not above one Grain odds in their Weights, the broader weighing about a thin Groat more than the narrower.

But because I would have no evasion, by supposing that that small distance between the Weights and the Hole, might give some more help and strength to the sustaining Pillar of Water by letting in some Air to lean upon the Water, thereby to enable it to keep the *Equipondium*. I took a very flat peece of Lead, with a hole in the Centre, no bigger than would let through freely that small Packthrid that suspended the Weight, and letting the Thrid through the hole, and pressing the Lead so exactly close, that no Air can pass to press upon the Water and Weight, but just that little passage for the

the Thrid which impended also in the very middle of the Weight, and thereby covering the Orifice, fitted to the one Inch weight; yet the *Equipondium* between both, continued exactly the same, as when both holes were left open, one of two Inches, the other of one Inch Diameter, at both times, the Vessels being perfectly full of Water.

And yet according to the supposition the supposed Weight of the Pillar of Atmosphere impendent upon the two Inch Weight, could not be so little as 200 pound, and the weight of Air pressing upon the Centre of the lesser Weight, and nowhere else, being not a quarter of an Inch Diameter, could not in proportion, weigh the 100<sup>th</sup> part of 200 pound, according to the supposition of this new Philosophy: And as this while the entire Superficies of the Water, discharged of all manner of Gravitation of superior Air, by the close cover that every where covers its Superficies.

I do not understand what evasion can be made, unless they will suppose that

the little hole for the Thrid, might by a strange Contagion, infect the entire Body of Water, with a new Gravitation, which nevertheless is so impossible, that it deserves no other answer than what our common sense allows; for this little pertuse, letting in that little Column of Ayr of a quarter of an Inch Diameter, impends not upon the Water, but is wholly received by the Brass Weights, upon whose Centres it leans.

So that the imaginary repercussive Column bearing up the Body into an Equilibrium, with the impendent Column of Air (admitting the Air should have such a Gravitation downward) must needs be a fiction upon a fiction, and therefore upon the whole matter laid together, both are fictions, and neither true.

2. But yet farther, if such a pressure upward of the inferior, or (as I have called it) the recyling Column of Air should be admitted, it doth wholly destroy the supposition of Gravitation of the superior Column of Air, because in

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in truth it renders the superior and in-  
ferior Columns of Air in a perfect *equi-  
librium*, every lower portion of Air  
checking and counterpressing the su-  
perior, with the same strength or *poten-  
tia sustinens*, that the superior portion  
chargeth the inferior with a *pondus ae-  
primens*.

(Fig. 5.) For suppose in the 5<sup>th</sup> Fi-  
gure *AB* were a Cylinder or Column  
of Air, reaching from *A* the Superficies  
of the Atmosphere, to *B* the Superficies  
of the Earth, and of 12 Inches diameter.  
If the portion *BEF* press with an equal  
*potentia* upward to the portion *AEP*,  
pressing with an equal weight down-  
ward; then these two portions of Air  
must be at rest, and *in aequilibrio*, and so  
far every divisible part of every other  
portion of the entire Column. And if  
the superior and inferior parts of the Air  
be by this means *in aequilibrio*, there can  
be no more Gravitation downward  
than upward; for all motion must arise  
from a disparity of weight and weight,  
or power and power, or power and  
weight. And therefore it is that al-  
though

although one Scale be charged with a hundred pounds weight, and the other Scale be charged with as much, the Scales will be at rest, because *in aequilibrio*; and by the advantage though but of a Grain of weight, added to one Scale, they will be put into motion, but not 'till then.

But then it will be required to make out that, upon the Supposition, that in our first Instance the greater Scale is kept *in aequilibrio* by the *pondus* of the superior, and the *potentia* of the inferior Air; that there will be the same *aequilibrium* between the *pondus AE F*, and the *potentia EFB*, upon or in every imaginary Superficies of the Column of the Atmosphere; as *EFCD*, or any other imaginary superficient, interjacent between the top of the Atmosphere *A*, and the Superficies of the Earth at *B*.

And surely there need not be much painstaken to prove this: for it is not the interposition of the Scale that makes the *aequilibrium*, but it is the counterpressure of the superior and inferior

G part

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part of the aerial Cylinder, which must needs be as effectual, and of the same kind and strength, if that Scale were removed out of the way; for though the interposition of the Scale hinder the contiguity of the upper and lower Cylinder, yet it contributes nothing to the Gravitation of the one, or pressure upward of the other.

Upon all which large digression, it seems to me that their very answer given, that the equal Gravitation of each Scale, the greater and the lesser, is by the equal pressure, *viz.* by reason the greater Scale as it is pressed by a greater aerial Cylinder, is also supported by a reversed or recoiling Cylinder of Air of equal sustaining force, though not of equal length with the superior, destroys totally their whole Supposition of the Gravitation of the Air, and renders their contrivance of the investigation of the weight of the Atmosphere by the *equipondium* thereof, to a Cylinder of 29 Inches of Mercury, utterly ineffectual.

And this it seems to me must be admitted

mitted, namely that the inferior Cylinder of Air recoiling and returning from the Earth, or any other solid Base, doth in truth sustain the impending *pondus* of the same Cylinder; and so there is an *equipondium* in any given or imaginary Superficies of Air between the *pondus deprimens* and the *potentia sustinens*, in the same perpendicular Pillar or Column: and so the Air becomes not heavy in it self, but yet that it gravitates entirely upon the Base, which is as it were the common *hypomoclian* that receives the impression of the impendent Air, and remits the *potentia* upward in counterpoise to it.

But then they say, that where in that Pillar of Atmosphere the Contiguity between the superior and inferior Column is interrupted by some intervening body: as in the first instance, by the intervention of the Scale the *equipondium* between the *pondus deprimens*, and the *potentia sustinens* of the aerial Pillar, is wrought by another means, namely the imaginary Pillars of Atmosphere circumjacent to that intercep-

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ting body: as suppose the three Columns of Atmosphere *AMBNCO*, and the Column *B* were cut off at *D* by any solid body (suppose the Scales, in the first instance;) yet the two adjoining Columns gravitate upon the inferior Pillar of Air (suppose it transverse,) and so give it as vigorous a repercussion as if it were in the same perpendicular Column: as in the figure,

Suppose *BH* be a Column of Air, impending upon the cube *H*, of six Inches square; *H* to be (for instance) a cube of Brass, some way there sustained; *EN* be the subjected Column of the same base, resting upon the Superficies of the Earth at *N*. Suppose also *ADm*, and *CFo*, be two lateral Columns of Air, pressing g upon a lateral or transversal Pedestal of Air *GIKL*, upon an imaginary Superficies, contiguous to the lower Base of the cube *H*, namely *GDFI*; here the Cylinders *HD* and *CF* press so strongly upon the entire transversal Base *GDEFGKLMO*, that the portion thereof *EN* is so strongly bound together, compressed, and fortified by  
the

the columns  $AD$  and  $CF$ , that it sustains the cube  $H$  as forcibly as it would have sustained  $BH$  an entire Column of Air, and so makes an *equipondium* and sustentation of the cube  $H$ .

(Fig. 6.) To examine the truth of this Supposition, let us suppose in this,  $A$  to be the base of the cube  $BCD$ , and  $E$  to be the bases of four Pillars or Columns of Air, contiguous to the four sides of this cube, equal in Base to it, and all these Bases, both of the cube  $A$  and of the four collateral Pillars of Air, to be upon the Superficies of the Earth. If  $A$  be removed higher, as to  $H$  in the former figure, the intermediate space between  $H$  (in the former figure) and the Superficies of the Earth, *viz.* the space  $HN$ , must be furnished with Air; now the Air, that must supply that space, cannot be immediately thrust in from the Pillar superior to the cube, *viz.*  $HB$ , because the descent of that Pillar of Air is obstructed by the interposition of the cube  $H$ , whereupon it is bottomed. Therefore of necessity each of the collateral Pillars of Air,  $BCDE$ ,

must thrust in an equal portion of their stock of Air, to supply the *interstitium* of *Hn* in the former figure; and then they will want so much of their Pillar, as will make good what they have thus contributed to fill that space. And this they are to borrow from the Pillar *HB*, that lay perpendicular to the cube, which now at this Elevation is *BH*: for as the cube it gradually raiseth from *N* towards *H*, the Pillar of Air incumbent upon it must be displaced, and what is so displaced, must be thrust into the lateral Pillars *B C D E*, to supply what they contribute to the rellicted space *HN*: and so in every Elevation higher of the cube *H*, so much space as it leaves, must be immediately thrust in from the collateral Pillars adjacent to the Tube, and must be made good in the same moment from the Air, thrust out of the perpendicular Pillar *BH*, by the gradual Elevation of the cube to supply the collateral Pillars. By this proceſſ of one part of the Air into another, to accommodate the motion of its parts, it seems evident, that there is not

that

that pressure of one part of the Air upon another, nor of all upon the common Base, that should be like so many Wedges to keep every portion of Air just in that place it hath ; or by Gravitation of one part upon another, to keep all the parts so tight one to another, as the *Supposition* imports. For then certainly there could be no motion or transmigration of one part of the Air into another, as we see there is not only in the Instance above given, but in a thousand more. The most forcible Winds could no more remove one portion of Air out of its place, then they could remove a Pyramid of *Memphis*, if this *Supposition* were true.

Therefore there seems to be very little ground for the late Masters of Experiments, to lay any weight of proof for their supposed Gravitation of the Air, upon the *Torriceilian Experiment* ; which, by what I have said, and shall hereafter say more at large, depends upon a clear other Solution.

And thus far in general, touching the Gravitation of Air in Air, or upon

any other Bodies ; wherein I have been  
the longer, because I shall perchance  
hereafter have recourse to some of the  
things said in this Chapter.

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## CHAP. V.

*Concerning the Gravitation of heavier  
Fluids upon those that are lighter.*

**T**HERE be those that contend, that there is no Body positively light, but only comparatively ; and that the Ascension of any Bodies is not from the intrinsick nature or quality of the Bodies themselves, but because others that are more heavy than those are, and more vigorous in their descent, do drive up those that are less heavy, by taking their places, and so force them to mount higher.

Whatever may be said in this respect, in relation to Air, yet surely it holds true in other grosser Fluids, and sometimes in solid Bodies also : Oyl is truly heavy, and yet driven higher by Water,

ter, which is ordinarily heavier than Oyl. And the same is true in relation to Water and Mercury, the latter driving up the former though a heavy Body, because not so heavy as Mercury. As I have before said, there is a double Disparity or *equipondium* of heavy Bodies; one in relation to their intrinsick or specifical weight, another in relation to the external or quantitative weight. Water is specifically heavier than Oyl, and Mercury than Water; whence it comes to pass, that a bulk of Mercury weighs more than a bulk of Water equal to the bulk of Mercury; one square Inch of Mercury, counterpoising near 14 square Inches of Water. But yet the bulk or quantity of Water may be so much, or so great, that it may overpoise the Mercury, two ounces of Water necessarily being more, and weighing double to an ounce of Mercury, though specifically heavier than Water.

And therefore in the consideration of the Gravitation of heavy Fluids upon lighter, we are to take these two parts into consideration. *viz.*

i. The

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1. The Gravitation of a Body, specifically heavier, upon or in a Body specifically lighter; as the Gravitation of Mercury upon Water, or Water upon Oyl or Air.

2. The Gravitation of a Body specifically lighter, yet in quantity or extrinsically heavier than the Fluid it presseth; as two pound of Oyl upon a pound of Water, and the like.

As to the former of these, there seem to be four ways, wherein the Gravitation or pressure of a Fluid upon a Fluid, specifically lighter, sensibly is exercised. *viz.* 1. By way of Penetration, 2. by way of perpendicular, or other descending Depression, 3. by way of lateral Pressure, 4. by way of Elevation of the lighter Body.

For the first, if a cube of 12 Inches square be filled 9 Inches with Oyl, and the other three be filled up with Water; though for the present the Water gently poured on will flote upon the Oyl, yet in a little time it will penetrate through it, and get under it, and the 9 Inches of Oyl will swim upon it, pres-

fing

sing its Superficies equally every where.

And so if a *lumen* be at the bottom of the cube, the Water first, and then the Oyl, will gravitate upon the Air, and press through it.

But if the top of the cube be closely covered, so that Air cannot get in above the Water, the Oyl will not descend through the *lumen*, the reason whereof is hereafter explained.

For the second, it is very plain, that the heavier Fluid will in some Instances press upon all the lighter, by a depression where it can have way; sometimes immediately, sometimes mediately, *viz.* notwithstanding the interposition of another body. For instance,

If an empty Bottle or Glass-bubble be placed in a vessel of Water, at a small depth, the Air included in the Bottle or Bubble, being a Body lighter than Water, will mount up to the top, if it be not kept down: for a small pressure of Water, though it press upon the included Air as much as it can, yet it cannot compress the Air so much, as to drive into it a quantity of Water, that may

make

make the Bottle and included Water equal in weight, or more in weight than the like bulk of simple Water; but if more Water be poured into the vessel whereby a weight of Water incumbent upon the narrow mouth of the Bottle or Bubble, is sufficient to compress the included Air, to such a contraction as may admit Water enough to make the Bottle, with the admitted Water to counterpoise more than a like quantity of simple Water, the Bottle or Bubble will subside, and sink to the bottom again; if the vessel be unladen considerably of its Water, whereby the Air included in the vessel being under a less pressure, can expand it self to its natural dimension, and thereby thrust out the Bottle so much of the included Water, as may render the Bottle, with the included Water, lighter than a like quantity or bulk of simple Water, equal to the bulk of the Bottle, the Bottle will emerge again.

This is the Experiment mentioned by *Shottus*, in his *Magia hydraulica*, part 3, l. 5, problem. 3. and the excellency

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Author of *Hydrostatical Paradoxes*, pag. 16, 53, 153. and elsewhere. The Experiment is certainly true, but the Solution thereof, and the Conclusion deduced from it, of the Gravitation of the upper parts of the Water upon the lower, is as I think wholly mistaken; for the reason of it is not from the imaginary Gravitation of Water upon Water, but of Water upon Air, which being a lighter body than Water, and capable of compression into a narrower compass, by the Gravitation of Water upon it, sustains that weight of the Water, and is compressed by it, and under it: it is not an effect of Gravitation of Water upon Water, which is of equal intrinsick or Specifical Gravity, but of Water upon Air, which is a lighter Fluid, and compressible. Again, this Gravitation of heavier Fluids upon lighter, though not immediately contiguous, is plain by this instance.

If a Cylindrical vessel of Water be of 6 Inches diameter, and there be a Rundle of Wood specifically lighter than Water of five Inches  $\frac{3}{4}$  diameter placed

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placed at the very bottom of it, the Water will drive it up to the Superficies of it, because it is a Body specifically lighter than the Water. But if there be a hole of five Inches diameter in the bottom of that vessel, and the Rundle be placed upon that hole, so that it somewhat overlaps the hole, so that the Water cannot pass out of it, and then it be filled up with Water; the Rundle will gravitate upon that hole, and the incumbent Cylinder of Water, commensurate in Base to that Rundle, so hard and close, that it requires a weight in a pair of Scales, neer commensurate to the weight of the impending Cylinder of Water, to raise it from the bottom; and the reason is, because the Rundle being the *Operculum* of that hole, through which the Water should pass into the lighter Fluid, the Air, and so is the impediment of the Waters passage, it gravitates upon it, because it hath mediately a lighter Element upon which it gravitates, namely the Air. It is *Stevinus* his Experiment, in his *practical Hydrostaticks*, upon his 10. Proposition.

And upon the same reason it is, that if a Glass-bottle filled with Sea-water, and stopped, be let down twenty fathoms into the Sea, it will not break, for it is filled with a Fluid of an equal weight, and so the external Water doth not sensibly gravitate upon it, for it hath within a sufficient power to resist the external pressure. But if it be full only of Air, and stopt, and let down as deep by a weight appended to it, (as it must) it will be broken by the pressure and weight of the Water immediately upon the included Air, which is lighter than so much Water, and so it presseth upon it, to drive it up, and crusheth the glass into it.

And this I take to be one reason, though not the only reason of the Experiment of the Valve, and likewise of Oyl thrusting up Water into a Tube, though of a specifically heavier consistence; and possibly may contribute something to the instance in the fourth Chapter, whereby a Syphon filled up with Mercury to the hight of the shorter leg, and immersed in Water, the Mercury in the shorter legg will subside a little, and ascend in the longer

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ger leg, although the Mercury be a Body 14 times heavier than Water; for in these instances, though the immediate contiguity be of the heavier Body to the lighter, as Oyl to Water, and Water to Mercury, yet in as much as the Mercury in both leggs is in *æquilibrio*, and the Air is behind the Mercury in the longer legg of the Syphon, and behind the Water in the Tube, the Water in the one case, and the Oyl in the other, doth in truth gravitate upon that Air immediately and effectively, rather than upon the intermediate heavier Fluid, especially if the body of the Air be any whit lower than the Superficies of the Water.

And thus far touching the Gravitation or pressure of a Fluid specifically heavier, upon a Fluid specifically lighter. I should now come to consider the pressure of a Fluid specifically lighter, yet with an excess of extrinsick or quantitative weight upon a Fluid specifically heavier; as two pound of Water upon a pound of Mercury, or two pound of Oyl upon a pound of Water: but be-  
cause

cause this and some other considerations, for the better clearing of the matters contained in this Chapter, may occur in the next, I shall renit it thither.

## CHAP. VI.

Concerning the Pressure or Gravitation of  
lighter Fluids, upon those that are spe-  
cifically heavier.

IT is certain, that every Body, whether the fluid, or solid, incumbent upon another, though that Other be a thousand times intrinsically and also extrinsically heavier than the incumbent Body doth gravitate and press upon it.

And therefore if we should suppose the Air to have a weight, but a thousand times less than the like portion of Water, a portion of one square foot of Air incumbent upon the Ocean, would have some pressure upon it: somewhat like the Problem that passed in the Affirmative among Fresh-men in the University, *That if a Horse had as much as he could carry, the addition of the weight of the thousandth part of a Fea-*

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ther would break his back.

But this is not the pressure or Gravitation I am speaking of, but such a Gravitation or pressure, as would put the subjected heavier Fluid into a sensible motion, or change of place or situation.

In a pair of artificial Scales, a pound of Feathers or Water, will equipondate a pound of Gold or Mercury, and 2 pound of Feathers will preponderate a pound of Gold or Mercury, notwithstanding the great disparity of their intrinsick weight.

And so in a Ballance that seems more natural, if a Syphon of half an Inch diameter, with one legg of six Inches long, and the other of thirty four Inches long, or more, be filled with Mercury to the height of the shorter legg, and then stopping the Orifice of the shorter legg with the finger, fill the longer legg with water, and then opening the Orifice of the shorter legg, the Water in the longer legg, though specifically lighter than the Mercury, will drive out the Mercury out of the Syphon, because the extrinsick or quantitative weight

weight of so much Water exceeds the extrinsick or quantitative weight of the subjacent Mercury.

If a cubick vessel be filled six Inches high with Water, and a Glass-pipe open at both ends be immersed at one end in the Water, and then superinfuse gently (to avoid disturbance) so much Oyl as will fill it up ten Inches, *viz.* four Inches of Oyl, and six of Water, the Pipe will be filled with Water neer to the level of the Superficies of the Oyl, but not quite level to the Superficies of the Oyl, it will want so much, as the like quantity of Water in the Pipe, is heavier than the like quantity of Oyl.

But if a Pipe filled with Oyl, and the upper end stopt with a finger, whereby the Oyl remains suspended in the Tube, or Pipe, and then the open end be immersed in the Water, the Oyl, while it obtains a Superficies in the Pipe considerably higher than the Water in the vessel, will subside 'till it come to such an Elevation above the Superficies of the Water, as is proportionable to the degree of specifical Lightness that it

H 2 obtains

obtains more than the like quantity of Water. And 'till then it subsides into the Water, because it hath a greater proportion of accidental weight by its <sup>so</sup> great Elevation above the Superficies of the Water: for the Elevation of one fluid body above another, gives the greater strength and swifter motion to its descent: whereby though a lighter body, it conquers the resistance of the Water of a lower Superficies.

Which is the reason, why a Tube of Water, four foot high, casts out more Water in the same portion of time, through the same *lumen*, than a vessel of the same diameter, and but two foot high, as I shall hereafter more at large obserue.

And this is the reason, why if a small Glass-Tube of six Inches long, be filled with Water, and the upper end being stopped with the finger, whereby the Water is suspended in the Pipe, and then the lower end being immersed in a vessel of Water six foot deep, and then the upper end unstopped, the whole Water in the Pipe will empty it self, 'till

till it attain a Superficies equal with the Water in the vessel, and will not be kept suspended in the Pipe by the resistance of the Water in the vessel, because if it obtain never so little more height in the Pipe than in the vessel, it hath a greater force to press downward, than the Water in the vessel hath strength to resist it.

If a Tube stopt at one end be filled with Mercury, and inverted in the open Air, the Air will ascend and terebrate through the Mercury as fast as that descends; so there is a kind of pressure of the Air *ascendendo* upon the Mercury, and thereby both the Air and Mercury obtain their several natural motions, this *descendendo*, and that *ascendendo*, whereby Vacuity is prevented, and the continuity of the parts of the Univerie preserved.

But if the Tube be very small, so that the Air cannot make its way through the lower orifice, to gratifie the Mercury with a descent, by that means the Mercury will remain suspended in the small Tube, and will not descend. The

like will happen, if such a small Tube be inverted into Water, the smallnes of the orifice not admitting the ascent of the Water to supply the space derelicted by a descent of the Mercury, the Mercury by force of the Catholick law of Nature will remain suspended, and its natural descent will be thereby superseeded.

So that the pressure or Gravitation of a lighter Body upon a more gross and weighty, will be occasioned, 1. Either by the Excess of the quantity and extrinsick weight of the Fluid specifically lighter, or 2. by the advantage of its Position or Elevation above the Superficies of the grosser Fluid, or 3. by the interposition of that common law of Nature, *ut evitetur vacuum*.

I shall conclude these general Observations, touching the Gravitation of Fluids, with that of the Valve, described by the Excellent Author of *Hydrostatical Paradoxes*, cap. ultimo, which was certainly a most ingenuous Experiment, though I am not at all satisfied with the Solution he gives of it, nor the

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Consequences deduced by him from it, touching the Gravitation of Water upon Water, (I mean sensible Gravitation) of the superior parts upon the inferior parts of the Water in free water.

And I am as much dissatisfied with that obscure Solution that is given thereof by the Learned Author of the *Enchiridion Metaphysicum*.

And because in this one Experiment duly observed and improved, many of the most abstruse Conclusions of Hydrostaticks may be cleared even to sense, I shall distinctly examine it, and the *phenomena* appearing in it.

The Instrument is thus made, according to that form which I made use of.

I took a hollow Tube of Laton, closely soldered at the Sides and joyns, of 39 Inches long, two Inches  $\frac{1}{2}$  in the diameter from the top to the bottom, with a Brass Valve at the end of it soldered to it, and a brass Sucker or Cover of the same fashion with those used in Pumps, only in Pumps the Cover or Sucker hath its head upward, in this Instrument the head is downward, whereby

it being inverted it falls out about an Inch below the Valve, but cannot fall out more, because stayed by the Frame of the Valve.

*Fig. 7.* This Sucker exactly covers the mouth of the Valve, that no water can come in, when the Sucker is closely strained up, by a string fastned to it in the inward pin of it, and so conveyed up through the Tubes. The Sucker thus fastned to the Valve or Box, and the Box solder'd close to the Tube, and the string conveyed up through the Tube to the open end of it, the Engin is formed according to the Figure in the Margin, representing *A* the Tube, *B* the String, *C* the brazen Box of the Valve solder'd to the Tube, *D* the Sucker or Cover of the Valve let down as low as it will go, with a Hook to hold any weight appended to it; which Sucker being strained up with the string, closely stops the lower orifice of the Tube, and Box of the Valve.

The Valve thus prepared, I strictly weighed the whole Valve, Tube, Sucker, and Thrid, with the wooden Nut

Nut upon which to roll the string, which weighed in all three pounds, two ounces and a half, or 50 ounces and a half.

The Sucker (for so I shall call the name of the Valve) weighed seven ounces and one half: and I was curious in weighing these things, because the great use of my Experiments consisted in the comparison of weights of the Instruments themselves, and the Water.

I took then a great glass Tube, of about five Inches diameter, and about two foot and a half long, and filled it with Water; though I used also a Leaden vessel of six Inches diameter, and four foot long, for the immersion of my Valve, which though it were deeper, yet the Glass vessel was more accommodate to my use, and served as well, and therefore I used it, and thereupon made those several Trials.

(Fig. 8.) 1. The Tube let into the Water, with the Valve open, would freely subside to the bottom of the Water, be it never so deep, the water getting up into the cavity of the Tube: for the

the *moles* of the Tube in that instance, is no more than the length and thickness of the Laton, the Brass Valve and Sucker; but the weight thereof being more than such a single *moles* of water, it must necessarily subside to the bottom of the water.

2. The Valve being drawn up, and sustained by the Thrid wound about the wooden Nut at the top of the Valve, whereby no water could come into the Tube, and then left freely to subside in the water, would subside to a depth of one and twenty Inches, and so swim erect in that depth of water, one and twenty Inches being immersed in the water, and eighteen Inches of the Tube being in the open Air, above the Superficies of the water.

And the reason is, because though the Laton and Brass be specifically heavier than Water, and therefore will sink, as in the last Instance; yet because now the Tube sits upon the water, as one entire Cylinder, and a Cylinder of Laton thirty nine Inches long, and two Inches  $\frac{1}{2}$  broad filled with Air only, is

not

not heavier than a Cylinder of Water of one and twenty Inches long, and two Inches  $\frac{1}{2}$  diameter, but do equipondere; therefore the Tube sinks no lower than one and twenty Inches to make an *equipondium* between it self and the like bulk of Water.

3. I therefore with all the exactness I could, measured the weight of the Water commensurate to a Cylinder of two Inches  $\frac{1}{2}$  diameter, and one and twenty Inches long, (the quantity of the Tube swimming in the Water.)

First therefore filling my Glass Tube very full of Water, I immersed the Valve open into one and twenty Inches deep; then at that depth lifting up the Sucker, and inclosing the one and twenty Inches of Water thus impri-  
soned in the Tube, and so taking it out closed; and then when out of the Glass Tube setting it to run into a vessel weighed before, I find the weight of the included Water weighed fourty and seven Ounces and half, or three pound wanting half an Ounce, which is about three Ounces less than the weight of  
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the whole Instrument. And this abatement it hath for the thickness of the Tube and Valve, because the water that is pressed upon by the Tube and Valve, and so raised out of its place, is pressed up by the convex or outside of the Tube and Valve, and commensurate to it.

Therefore again straining up the string of the Valve, whereby no water could get in, and filling my Glass Tube of restagnant water to the very brink or brink of the Glass, and then I immerfed my Tube into the water to one and twenty Inches depth; the Valve strained up by the string, and preserved the water that was impelled up by the immersion, and thrown over: and this I likewise exactly weighed, and found to weigh near upon the point of fifty one Ounces  $\frac{1}{2}$ , or three pound three Ounces and half, which is about an Ounce more than the weight of the Engine; which disparity might happen for want of exactness. But the summe of it is, there is no considerable difference between the weight of the whole Engine, and the weight of so much Water, as will

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countervail in quantity to so much of the Engine or mingled Cylinder of 21 Inches thereof immersed in water.

Which gives us a plain account, why and how much a Body lighter than the like quantity of water, will sit above water, and why, and how it comes to pass, that some Ships draw more water than others, and how much burthen they will bear, before they will sink, *viz.* if the Ship or Barge with all its Cavities and Lading, be not heavier than such a solid bulk of water, as equals the whole *Moles* and Cavity of the Ship or Barge, the Barge will live, though it draw water neer to the very top of the fides of the vessel. And if the Ship or vessel be lighter than such a *moles* of water as equals the whole continent of the Ship or Barge, so much of the Ship or Barge will emerge above the water. For in the instance in hand, in as much as 21 Inches of immersion of the Tube, was the product of its whole weight of three pound, two Ounces and half; and those one and twenty Inches thus immersed, were

equal

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equal in weight to a bulk of Water  
commensurate to that Cylinder of 21  
Inches immerfed, the rest of the Tube  
viz. 18 Inches, did flote erect upon the  
Water ; for if it should have subsided  
the *moles* of Water lifted up out of its  
place would have exceeded the weight  
of the whole Valve, *viz.* three pounds  
two Ounces , and half, which by the  
rules and Laws of *Hydrostaticks* it may  
not do,

4. If the Sucker be drawn up , and  
then immerfed so low, that the portion  
of Water impelled up by the Tube , do  
exceed the weight of the Sucker, *viz.*  
ounces and half , the Sucker will be  
sustained by the pressure of the Water  
upon it : as suppose in the 8<sup>th</sup> Figure it  
were pressed even to *L M*, or *N O*, which  
is nine Inches below the Superficies of  
the Water. And the reason is , because  
the Sucker is now the moveable Basis  
of the included Column of Air , which  
is a light Body, and so gravitates no  
thing upon the Water ; and the Sucker  
being as it were the Basis of that Co  
lumn, and moveable, weighs but seven  
Ounces

Ounces and half, and the Tube subsiding as low as *LM* or *NO*, presseth up a portion of Water of double that weight, and this portion of Water bears against the Base or Sucker more forcibly and powerfully than the Sucker can bear against it.

5. But if the weight of a *moles* of Water, commensurate to so much of the Tube as is immersed in the Water, be less than the weight of the Sucker, the Sucker by its own weight will subside, notwithstanding its immersion into Water.

By iterated Trials I found, that if I kept up the Sucker by the Thrid, and immersed it to ten Inches, or more, and let go the string, whereby the Sucker, though now at liberty, would yet not subside, but remain suspended by the pressure of the Water upon it; yet if I gently raised it up, 'till it came only to an immersion of four Inches and half, the Sucker would subside; wherefore closing in the included Water at that depth, by lifting up the Thrid, and so closing the Sucker, I weighed that inclosed

112 Observations touching the closed Water, and found it just weighed as much as the Sucker, *viz.* seven Ounces and half, and there could be no considerable odds here by the thickness of the Tube, the immersed portion thereof being but small, and the sides very thin.

6. But because I would pursue the proportion to the uttermost, I kept up the Sucker with the string, 'till the Tube came to the immersion of four Inches and half, and then subsided; I then inclosed that four Inches and half of Water, and kept up the Sucker, and immersed the Tube with the inclosed four Inches and half of Water, and gently raising it up, it subsided at nine Inches immersion.

And the reason is the same as before, for the included cavity of nine Inches, had in it four Inches and half of Air, which weighed nothing, and four Inches and half of Water, which as before is shewn, weighs seven Ounces and half, and the weight of the Valve which weighs also seven Ounces and half, the whole weight of the included Air, Water, and Valve, weighing only fifteen Ounces

Ounces, and a Column of Water of nine Inches high, and two Inches and half broad, equal to the cavity of the immersed nine Inches of the Tube, would have been of the same weight with the four Inches and half of included Water; and the Sucker, whose weight is now to make good the uppermost four Inches and half of Airy space, and so there is an *equipondium* between the included four Inches and half of included Water, together with the weight of the Sucker to a Cylinder of Water of nine Inches long, and two Inches and half broad, commensurate to the like cavity of the Tube, and the like quantity of nine Inches of Water, impelled out of its place by nine Inches of the Tube immersed below the Superficies of the Water.

And the same rule and proportion will hold upon the like quantity of more Water taken in, and a deeper immersion, allowing the same thing for that thicknes of the sides of the Tube, which upon a deep immersion is more considerable than upon an immersion of four Inches and half.

7. And although I did not make an actual trial how much weight in an empty Tube, deeply immersed, would be sustained by the Sucker without subsiding; yet by this proportion it is easie to be calculated, *viz.* that the Tube being kept empty, and immersed to any depth below four Inches and a half, the Sucker will sustain without subsiding such a weight, as is equal to a portion of Water commensurate in bulk to so much of that empty Cylinder in the Tube, as is depressed below the Superficies of the vessel of restagnant water.

So that if the empty Tube will sustain seven Ounces and half (the weight of the Sucker,) at four Inches and half immersion, it will sustain fifteen Ounces at nine Inches immersion, thirty Ounces at eighteen Inches immersion, sixty Ounces at thirty six Inches of immersion; for an empty Cylinder of thirty six Inches long and two Inches and half broad, will contain a Cylinder of Water of sixty Ounces weight, allowing somewhat as before, for the thicknes of the sides of the Tube.

8. In this it appears that the Water doth not gravitate upon the Water, but gravitates upon a Body lighter than it self, though this Gravitation be immediately upon a Body as heavy or heavier than it self. When there is nothing but Air in the Tube, the Sucker is the moveable Basis of that Air, and it gravitates immediately upon the Sucker, but mediately and remotely upon the Air above it, when there is a little column of Air in the top of the Tube, and below the Superficies of the Water, and under that a column of Water, and under that the Sucker. If the whole Concrete be lighter than so much Water, it gravitates upon the Sucker, Water, and Air, because though the Brads be specifically heavier than Water, and the Water over it specifically of an equal weight with Water; yet the quantitative or extrinsick weight of both is less than the weight of so much Water as is equal to a Bulk or Cylinder of Water, entirely commensurate to the quantity of the Air, Water, and Sucker, below the Superficies of the Water.

9. That when a Body lighter than so much water is within the compass of the Superficies of the body of restagnant water, the water presseth upon it upward, impelling it up, as here upon an immersion of the Valve laden with four Inches and half of water, and likewise with the weight of the Sucker, into nine Inches of water, an equal quantity of water to the *moles* of the immersed Tube, *viz.* nine Inches sustains the Sucker, notwithstanding its own weight and the weight of four Inches and half of water incumbent upon it, because those two weigh but fifteen Ounces, and nine Inches of the exterior water weighs as much, and so there is an *equi-pondum* between the *potentia sustinens*, and the *pondus eprimens*.

## CHAP. VII.

*Concerning the Space derelict by the descent of the Mercury in the Torricellian Experiment, and what it is not, or what it is that fills it.*

Having gone through the two premised Considerations, preliminary to this Inquiry, I now come to the third General, namely what it is that fills the space derelict by the descending Mercury in the Tube. And first I shall consider, whether it be a Space purely void of any Body. 2. If it hath some Body that fills that Space, I shall consider what it is not that fills it. 3. I shall consider what it is that most probably may be thought to fill it.

First therefore I say it is not Nothing, or a pure Vacuity, but it is some corporeal substance that succeeds in the head of the Tube, derelict by the Mercury.

And here 1. I shall not enter into  
I 3 any

any large debate touching the possibility or impossibility of interspersed or coacervated vacuities in any parts of the Universe; neither 2. shall I insist upon those obscure evidences of the in-existence of a Body in that derelicted space; as namely the communication of Sounds, or the reception of Light, or transmission of Colours, or other visible *species* in or from it. But 3. I shall ground my Conclusion from those more sensible and apparent *Phænomena* which do evince it; namely, those sensible plain Effects in that seeming empty Space, which could never be produced but by the contiguity and presence of some corporeal substance, filling that supposed empty space.

1. It is most evident to any man's sense that will but try it, that if the Tube be open at both ends, and filled up with Mercury, and then one end stopped with the finger, and the other end inverted and immersed in the stagnat Mercury, whereby it descends from the top of the Tube, a strong and sensible Attraction is wrought upon the pulp of

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the upper finger that closeth it, which continues and grows more and more forcible, sensible, and evident, the farther the Mercury is removed from the upper end, and approaching to its usual station of 29 Inches.

This Attraction is impossible to be without the contiguity of some Body to the pulp of the finger, which cannot be the Mercury it self, for That in a Tube of four Foot long is removed above a Foot distant from the finger, by that time it comes to its station; yet all this while the strong suction upon the pulp of the finger continues.

And they that go about to tell us, that it is not Attraction or Suction by any power within the Tube, but by the pulsion of the incumbent column of Air, need no other confutation than their own sense to assure them the contrary; for most evidently the force that the finger feels is from within, and not from without: and this Solution by Pulsion, is given only to gratifie that imaginary Supposition that some have entertained of the Aery Column, whereof in due time.

2. The instance of *Honoratus Fabri* in the 6<sup>th</sup> of his *Physical Dialogue*, (though not tryed by my self, yet asserted to be tryed by him) puts it out of question. If at the upper end of the Tube there be fixed a folded Paper, which may be extended by any force, but otherwise contracting it self (in the manner of Paper-Lanthorns made by School-boys), this *plicata charta* will by the subsiding of the Mercury be unfolded and extended, which could not be, but by the contiguity of some Body unto it.

3. The instance given by *Linus*, and improved by *Fabri, ubi supra*, of an empty Bladder close tyed, and fastned to the upper end and the inside of the Tube, extended after the descent of the Mercury, and again contracting it self to its former dimension by the inclination of the Tube, makes it plain, that this Extention is the effect of some included Body in that relited space, which is not the bare gros body of the Mercury, which is removed a Foot or more below it, and in which there is not so much as pretended to be any Magnetick quality

lity in relation to the Bladder.

And they that tell us it is the Elatery of the Air included in the Bladder, that extends it by a natural expansion, tell us an invention fitted to serve the Supposition of the prodigious natural Elatery of uncompress'd Air, whereof hereafter.

It is true, that if this Bladder hath any little orifice, or hole open in it, the distention will not ensue; but the reason thereof is evident, (not upon their Supposition of the impediment of the natural Elater of the Air included in the Bladder,) but because in that case the Attraction is made as well upon the inside of the Bladder through the orifice, as upon the outside, and so the Bladder obtains the same consistence as at first; but where there is no orifice, the Attraction can only be but upon the outside of the Bladder, upon which there necessarily follows an Extension of the Bladder, and a necessary Consequent thereof is a Dilatation and Expansion of that little particle of Air included in it, to the uttermost dilatation that

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that that attractive force can give it, or  
produce in it, which is not by natural  
Elasticity, but by forcible Tension.

Upon these and the like sensible and  
plain Instances it seems undeniable by  
any, that will not abandon his own  
sense, that it is not Nothing that is in  
that derelict space, but it is some bo-  
dily substance; for otherwise it were  
impossible that those, and the like sensible  
effects could be produced: for regular-  
ly, all natural bodily effects are wrought  
by a contact of some Active body up-  
on the Patient.

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## CHAP. VIII.

*Concerning the Body supplying the derelict  
Space in the top of the Tube, and first  
what it is not.*

IN the former Chapter I have conclu-  
ded, that it is some corporeal Sub-  
stance in the space derelict by the  
Mercury in the top of the Tube, though  
not conspicuous to the sight: and now

I shall consider first what it is not, and  
2. what it seems most probably to be.

There are two general Opinions touching it: one that supposeth it is some Body that comes *de foris* into that derelicted space; and those again fall into several Opinions; some suppose it to be the forreign Air that pervades either the Pores of the Glas, or the Mercury; others that it is *Æther*, or Ethe-real matter, or (which is a differing kind of Explication of the same thing) that it is *des Cartes* his *materia subtilis*, or his Third Element, the *ramenta* or filings of Atomical Bodies. The other gene-ral Opinion is, that it is a bodily sub-stance, not coming *de foris*, but arising from the included Mercurial Body.

Now as to the former of these gene-ral Suppositions that it comes *de foris*, I shall discharge the question from that Supposition of *des Cartes* his *materia subtilis*, for there is no probable evidence of any such Principle, it being only an imaginary substitution; and if it be any thing besides imagination, it seems to be but Air, or at most that subtler part  
of

of the Universe called *Aether*, which possibly may be nothing else but the subtler and more refined parts of the Air, disengaged of those vapours or *Effluvia*, which obtain ordinarily in the inferior parts of the Air, commonly called the Atmosphere.

And therefore one method of discussion of this question will possibly be applicable to both these Suppositions, of the admission of forreign Air or *Aether* into the derelict space in the Tube above the Mercury.

If therefore it be forreign Air or *Aether* that occupies that place, it must be upon one of these accounts, *viz.* either that the Air or *Aether*, upon the account of its own subtlety and tenuity, freely and of its own accord pervades the Glass or the Body of the subsiding Mercury, and so takes up the derelict room; or else that although of its own accord it would not penetrate through either of those two Bodies, yet that it is as it were forcibly strained through the pores of the Glass or Mercury, and by the subsiding of the Mercury, and the

re-

resistence of Nature against Vacuity, percolated into that vacant space.

As to the former of these Solutions, it seems altogether unreasonable to imagine any free or unconstrained penetration of the Air, or *Aether*, into that space: for although the Glass, and likewise the Mercury are not altogether destitute of Pores, yet they do not run in a straight uninterrupted line, but are crossed and obstructed by interposed filaments, that must necessarily stop the free passage of the Air or *Aether* through them, so that they are not wholly pervious to those Bodies.

And that this is so, appears by this one instance, which is as good as a thousand.

If it were true, that the Air or *Aether* did freely pass through the supposed pores of the Glass or Mercury, the Mercury would never be suspended at twenty nine Inches, but would wholly sink into the vessel or stagnant Mercury; for the insinuation of the Air or *Aether* through the Glass or Mercury, would supply the vacuity, as well below twenty

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ty nine Inches as above, and so accom-  
modate the derelict space in all its E-  
vacuation, as well as in part, and like-  
wise would accommodate the Mercury  
in its natural descent without any in-  
convenience; and yet we find, that a  
twenty nine Inches the Meccury keep  
up, and subsides not.

And that which puts it out of que-  
stion, that the natural irruption of the  
Air or *Aether* fills not this Space, is this  
let but the least orifice, no bigger than  
the point of a small Needle be made in  
the top of the Glass Tube, the Mercury  
would entirely descend, and not be re-  
stained at twenty nine Inches, because  
here is an admission of forreign Air to  
supply the space derelict by the Mer-  
cury, as fast as it descends. And the  
same effect would follow, if the Air or  
*Aether* did penetrate through the small  
pores of the Glass or Mercury. And since  
we see it doth not obtain that effect, we  
have reason unquestionably to conclude  
it hath not any admission *ab extra*  
through those supposed pores of either  
of those Bodies.

It remains therefore to be considered, whether that by the great contention of Nature against Vacuity, and the forcible stress of the Mercury on its descent downward, there be not a kind of forcible straining of some Aery or Ethe-  
real Particles through the Glass, or at least through the Mercury, which is partly expoled to the external Air.

I answer, first, as touching the permeating of Air or *Aether* through the Glass ; it seems utterly impossible, for the reason before given. Indeed the vigor of Light or Fire penetrates the Glass, not (as it seems) by any transiition of any Corporeal Substance, but only of a *vis*, *virtus*, or *vigor*, which communicates the quality of *lumen* or *calor* to the Body that it finds there, but conveys not any Corporeal Substance transmitted through the Glass.

Secondly, as touching the supposed straining of the Air or *Aether* through the Mercury, and discharging it in the summity of the Tube : I must needs say, that in some instances the forreign Air will make a visible perforation and  
tran-

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transition through the body of Mer-  
cury, as well as of Water.

If a Tube of Glafs, fealed at one end, be filled with Water, or Mercury, and then inverted in the free Air, (unless the Tube be extremely small) the Mercury will subside and descend by the fides of the Tube, and the Air ascend through it in a Pillar or Bubbles in the midſt, in the very ſame ſucceſſive order as the Mercury descends; the Mercury thus laterally descending, making way for the aſcent of the Air, and the Air in the ſame ſucceſſive moments aſcending, filling the room relighted by the Mercury: and this aſcent of the Air is evident and conſpicuous to the Eye.

But in the matter in queſtion, there ſeems to be no ſuch pervading of the Air through the pores of the ſuspended or reſtagnant Mercury.

First, if ſuch a transition of the Air were through the column and reſtagnant Mercury, it muſt needs (as in the former caſe) make a viſible appearance thereof in the Mercury, and raiſe ſome tumultuous *phenomena* in it, but no ſuch thing

thing appears to the eye in the *Torricellian Experiment*!

Secondly, the difficulty of the *transitus* of the external Air through the Mercury in the *Torricellian Experiment* is infinitely greater than in the instance above given: for in that instance the Air hath but one motion upward, and is assisted in it by the Mercury giving way to it. But here in the *Torricellian Experiment*, there must be a double intention and motion of the Air, *viz.* 1. It must *descendendo* terebrate through the stagnate Mercury, and then when it hath passed that difficulty, it must terebrate through the erect column of Mercury *ascendendo*. And it is not conceivable that the pores of the erect and stagnate Quick-silver, crossing one another in respect of their position and situation, can accommodate such a crooked and contradictory motion of any particle of Air.

Thirdly, again it cannot reasonably be thought, but that if there were such a transition of the Air through the stagnate and suspended Quick-silver,

K but

but it should be as reasonable to continue after the subsiding of the Mercury to twenty nine Inches, as before, the passage remaining still open through the Mercury. The consequence whereof would be, that the whole Tube would be supplied with Air, and consequently the whole body of Mercury be emptied into the restagnant vessel; as when a little Pin-hole is made in the top of the sealed Tube.

Fourthly, again if it were forreign Air or *Aether* that were admitted, it is not easily conceivable, how the finger stopping the upper orifice of the Tube, should be attracted inward, or the Bladder distended, as in the former Chapter; for the immision of forreign Air through the Mercury, or Glaſs, must rather contract than expand the Bladder, by the accession of an external compression from it, as will happen upon the least hole made in the Tube. Upon these and the like reasons it seems to me altogether improbable, that the derelicted ſpace should be filled with Air or *Aether* from without, by an immision

mission of it through the Glass or Mercury.

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## CHAP. IX

*Concerning the other Supposition, namely,  
that this derelict Space is filled with  
a Body ab intra.*

Since therefore I have concluded, that this derelict Space in the Tube is filled with a Body, and that That Body comes not *ab extra*, and there is no Body within to supply that room but the Mercury, or that which must be extracted from it: and in as much as the Mercurial consistence it self is descended to twenty nine Inches, it remains necessary that this Body that fills the derelict *interstitium*, must be a subtle body extracted from the Mercurial body. But how, or in what manner this is effected, is inquirable.

And now before I come to the Inquiry it self, I must take notice that there are three kinds of instances of the *Torricellian Experiment.* K 2 I.

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1. The first is, where the Tube is just twenty nine Inches and half long, and sealed at the top, and then filled with Mercury, and inverted into restagnant Mercury, without any admision of any Air; and though that be difficult to effect, yet if it be effected, there is no vacant space left, but the column of Mercury adheres to the top of the Tube, it being its just station.

2. The second is, when the Tube is above twenty nine Inches and half, suppose four foot long, sealed at the top, and filled up to twenty nine Inches and half, and then inverted, whereby the Air riseth to the top of the Tube, and the Mercury subsides not only to twenty nine Inches and half, but much lower, namely as far as that column of Air in the upper end of the Tube is by tension extendible by the force of the descension of the Mercury, which being about eighteen Inches of Air, possibly by the descent of the Mercury will be extended to above twice the length, whereby the Mercury may subside it may be to about fourteen Inches, (I am not curious in the Calculation.)

3. The

3. The third Instance is, where the Tube for the purpose is four foot long, sealed at one end, filled with Mercury without any portion of Air admitted, whereby the Mercury inverted into a vessel of restaguate Mercury, subsides to twenty nine Inches and half, and so leaves about eighteen Inches deserted by the Mercury.

And though in the pursuit of this discourse there will be use of the discussion of the two former Instances, yet in this place only the last of the three yeilds the Inquiry, namely what it is that fills that space derelicted by the Mercury.

And I adventure to conclude, that it is a subtle Corporeal Substance extracted from the gross Mercurial Body, or forced out of it.

The methods of this Séparation are two, the one proposed by *Linus*, the other offered by others.

That of *Linus* (if I understand him aright) seems to be this: namely that by the descent of the Mercury, and *in obsequium naturae universalis*, for preventing of Vacuity, there are taken away succel-

sively from the superficies of the Mercury certain scales as it were of an indivisible profundity, and these are rarefied into a subtle invisible consistence or vapor, which supply that deserted space, and make up a kind of *funiculus* that suspends the Mercury to the Tube, and is coextended to the whole vacant space gradually, as the Mercury descended.

This seems to be somewhat difficult, especially considering the heterogeneous parts of the Mercury, whereof some seem to be of that solidity, that are incapable of such a separation.

Besides it would be difficult to conceive, how these superficies of this Body should in a moment, by the inclination of the Tube, be rejoyned to the Mercurial body and consistence; and in a moment again, upon the re-erection of the Tube to its perpendicular posture, be rarefied from so gross a consistence into so subtle and invisible a nature; both which will be the effects, one of the inclination, the other of the re-erection of the Tube.

Therefore I shall rather choose another,

ther, and, as I think a more accountable Solution.

It is plain to our daily observation, that all Bodies, especially such as are liquid or fluid, do continually send out from them certain subtle *Effluvia*, scarce perceptible to the Eye, but manifest in their effects. It is also evident, that all Bodies, especially fluids, and such as consist even of Heterogeneous parts, by strong agitation, by heat or motion, have many of their parts resolved into a more subtle consistence, than appeared before such agitation. Thus Water by heat or violent motion resolves it self into vapours or steam.

Mercury is of it self an heterogeneous body, consisting of some more solid, other more fluid and moist parts. If any man shall take the pains violently to shake Water and Mercury together, and throughly wash it, he shall find the shining parts that are the more fluid, severed from little black or dark granules like Sand, which will gradually subside visibly in the water into a heap, before the nimble shining particles united

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to them, and also certain Airy corpuscles  
rising to the top of the Water.

And as thus by the separation by wa-  
ter, so by a strong heat the Mercurial  
Bodies will be in a great measure re-  
solved into a fume or smoak, which ne-  
vertheless (as is said) will coagulate a-  
gain, and assume the former Mercurial  
consistency, at least in a great measure.

Mercury, though it be one of the col-  
dest Minerals, doth contain and send  
out from its body certain *Efluvia*, and  
noxious vapors, as they experiment  
that are conversant in Mines of that  
kind.

If any man observe the descent of the  
Mercury from the top of a long Tube in  
the *Torriceilian* Engine, he shall find  
the descent not so quick and nimble, as  
if both ends were open, and (contrary  
to the usual acceleration of descending  
heavy Bodies) more and more slow and  
faint, the nearer it comes to its station;  
which is an evidence that there is a  
*lucta* and contention in Nature, while  
it extrates, separates, and abrades the  
parts, that must supply the derelicted  
space,

space, and likewise in giving them a tension or dilatation answerable to it.

They that have exercised much the *Torricellian* Experiments, and those of the like nature with Mercury, have apparently found that the frequent iteration of these Experiments have much imbas'd the nature of Mercury, depurating it from those subtle steams and *Effluvia* that naturally attend it, and thereby the very Mineral it self becomes more vapid, discharged of its subtle Spirits or parts, and rendred less useful not only for Medicinal but Mechanical Experiments: whereby it appears that it loseth somewhat of its more subtle substance by iterated Experiments, and will prove more sluggish. For this I call to witness *Honoratus Fabri*, in the latter end of his *Philosophical Dialogues*, and some of our own Nation that have asserted the same, and in my own Experience I have also found it true.

It is certain, that when Mercury, or any other fluid falls in a Tube, the higher it falls, the more force it hath, and the greater compression the lower parts receive

receive from the upper ; for all Bodies that descend, descend with the greater force, according to the altitude of their descent, as I shall have occasion hereafter to observe.

The descent therefore of Mercury in a Tube of six foot high , is much more powerful than a descent from four foot high ; and That than a descent from three foot, or twenty nine Inches high, and consequently the compression of the inferior parts by the superior ; and in a greater descent than in a less. the agitation of all is the more vigorous

And yet (as I before said) it is evident in the descent of the Mercury from a Tube of four foot high, stopt above, and immersed beneath in stagnat Mercury, is not without a *lucta* or contention ; and although the higher the Tube is, the descent at first is more quick, yet it gradually grows slower and slower, 'till it come to the common station of its suspension, *viz.* twenty nine Inches and half, or thereabouts. But yet even in the first efforts of its motion, it is not so quick or violent, as its own natural motion

motion would otherwise attain for it, receives a *remora* and impediment by the difficulty of separating of its subtle parts.

These things are apparently true to any man's observation, and from these observations I frame these Conclusions.

1. That the substance that fills the head of the Tube, as the Mercury descends, are the *Effluvia*, steams, or subtle vaporous parts of the Mercurial Body.

2. That these steams or vapors are separated from the Mercurial body by a double immediate means. First by way of Expression, or driving them out by the strong descending motion of the Mercury, the compression of the inferior parts by the superior, and the great agitation of its parts; the grosser Mercurial parts coming closer together, and driving out the more subtle and vaporous parts. Secondly by way of Extraction, or straining out those parts that are more subtle and fluid, and capable of Expansion, and from hence ariseth that *lucta* and *mora* in the descent of

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of the Mercury, by the straining of the  
Mercurial vaporous bodies, and raking  
them out. And

3. That those parts thus extracted  
are dilated and extended to as great a  
amplitude and tension, as the strength  
of the descent of the Mercury, and the  
amplitude of the space it leaves re-  
quires; which tension also contributes  
much to the retarding of the swiftness  
of the Mercurial descent.

4. And because at twenty nine in-  
ches and half, the weight of the Mer-  
cury decreaseth to such a state, as cannot  
work any farther separation of subtle  
matter to supply any more room, it  
stays there, and descends no farther, not  
having strength enough at that height  
to separate any subtle matter from it-  
self, to supply the space it should leave  
by such descent.

5. Although the immediate cause  
of the separation of the subtle matter of  
Mercury, and the expansion or dilata-  
tion thereof, be the force of the descent  
and weight, and pressure of the Mer-  
cury, yet this is performed *in obsequium*

universalis naturæ, for the avoiding of the dissolution of Continuity, or, which is all one, *ob fugam vacui.*

6. And the same reason *mutatis mutandis* will be applicable to the suspension of water in a Tube, which will be suspended at 31 foot, or thereabout, as Mercury is at twenty nine Inches. For by the same means that subtle parts are separable from Mercury, subtle parts are much more separable from Water, by a strong tension or compression of it, as appears by the raising of innumerable Airy Bubbles out of a vessel of Water, by the strong tension of the Air in the Air-pump, large instances whereof appears in Mr. Boyle's Experiments, 19, 21, 22, 23, &c.

The whole process and model of the suspending of Water in a long Tube above one and thirty foot long, and how it subsides to about one and thirty foot, and no longer. *vide apud Shottum* in his *mirabilia Vari lib. 3. cap. 2. in Experimento Roberwalli Magnani & Bertii.*

And now if it be said, suppose the Tube were seven, eight, or ten foot long,

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long, or if it were three or four foot  
long, and the upper end were a Bolt  
head, that should contain three or four  
pound of Mercury, this would subside  
to twenty nine Inches: where should  
there be *Effluvia* to fill so great a space?

**I**anſwer: the more Mercury descends  
to twenty nine Inches, the more *Effluvia*  
there will be to fill that place than  
the Mercury left, and so the Supply will  
necessarily be as liberal, as the space the  
Mercury took up before its descent to 29  
Inches and half, because there is a greater  
stock of Mercury to yeild it, and  
a greater continuation of motion to ex-  
tract it. And there seems no necessity  
of any great quantity of subtle matter  
to be driven, or driven out of the Mer-  
curial Mass, because it is a substance ca-  
pable of dilatation to a greater ex-  
pansion than its own natural consistency,  
by the subsiding of the Mercury.

**Mersennus** tells us, that by experience  
he finds, that by a strong tension of a  
particle of Air, it is capable to an ex-  
pansion of neer seventy times the space  
of its ordinary consistency, as in *Æoli-*  
- *piles*

piles, and the like Engins. And we see that if an Inch of Air be left in the top of the Tube, it will by the bare force of the descent of the Mercury be expanded to neer ten times its former space. And upon the same account it will fall out, that the quantity of one Inch of those Mercurial effluxes will, by the descent of the Mercury, be stretched out and expanded to fill the derelicted space of the Mercury, far exceeding their common and ordinary extenſion,

*Fig. 9.* The most considerable Objeſſion against this Suppoſition ſeems to be this. That if the Tube of four foot long filled with Mercury, and then immeſſed in reſtaignant Mercury, where-by the Mercury ſubſides to twenty nine Inches, and leaves a ſpace of about 18 Inches ſeemingly empty, yet if the Tube be inclined, as in *B*, to an altitude pa-rallel to twenty nine Inches of vertical hight. The Tube *B* will be as at firſt filled with Mercury, drawn or driven out of the reſtaignant vessel, and no va-cant ſpace left; and if again it be ere-ated perpendicularly as before, the Mer-  
cury

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cury will subside as before, *viz.* to twenty  
nine Inches; what becomes of the  
subtle exhalation upon the inclination  
of the Tube?

I answer, it is true, that by the incli-  
nation of the Tube the Mercury that de-  
scended out of the Tube into the re-  
stagnant vessel of Mercury, will be  
drawn up into the Tube, and the Tube  
filled again with it, the same by the in-  
clination of the Tube becomes of less  
accidental weight. But this doth no  
way crois what I have supposed: I  
therefore answer,

1. That by the inclination of the  
Tube at *B*, the Mercury doth not press  
so urgently upon the subjected Mercury,  
as it doth in its perpendicular position;  
for it is sustained according to the laws  
of Staticks by leaning upon the incli-  
ned Tube, and presseth not so forcibly  
upon the fund or Base.

2. That therefore the pores or con-  
sistency of the Mercury in the inclined  
Tube are more lax and open, and regain  
more of that natural texture of its Mer-  
curial consistence, and receives into it  
again

again those connatural steams or effluxes, that were shot or drawn out of it by its former compression, and the attraction made upon it by reason of its descent.

3. And by its re-erection unto its former perpendicular posture, they are again thrust or drawn out by its contraction and compression, and the forcible attraction wrought upon it by that Catholick Law of Nature, preserving the continuity of the Universe.

And that which seems evidently to evince the truth hereof, is the instance given by the Excellent Author himself, in his 20, 21, 22, 23, but especially in his 24<sup>th</sup> Experiment of the Air-pump; wherein the violent attraction of the Pump, and the strong tension of the Air included in the Receiver, raiseth from the bottom of an included vessel of Water Oyl, but especially Spirit of Wine, great Bubbles to the top of the vessels, (which is performed either by that forcible extraction of the more subtle and Aerial Particles included in that liquor, or by subtilizing or rarefying some

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parts of the liquor. But as soon as that  
strong tension and attraction by the  
included Air is taken off, by the immis-  
sion of fresh Air into the Receiver, the  
Bubbles subsided, and the subtle parts  
included in it were again rejoined, and  
reunited to the same liquors from  
whence they were exhausted, which  
by a new tension wrought by a new re-  
peated suction of the Pump, might be  
again extracted.

And thus far touching the third Ge-  
neral, namely what it is that is inclu-  
ded in the space derelict by the Mer-  
cury.

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CHAP.

CHAP. X.

*The enquiry touching the Cause that keeps the Mercury suspended in the Tube at 29 Inches, or thereabout; and first what is not the Cause of this Suspension.*

THE consideration of what it is that remains in the derelicted space, in the upper end of the Tube, and the consideration what it is, that keeps up the Mercury to twenty nine Inches, or thereabouts, are of two several kinds: for although the former Consideration is necessary to be known, before we can arrive at a reasonable discovery of the latter; yet it would not be simply of it self impossible, that there should be such a corporeal substance, as I have above conjectured, in the derelicted head of the Tube, and yet the Mercury might wholly discharge it self in the restag-nant vessel, which yet we see it doth not.

Therefore having in the former

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Chapters finished the former, which was the fourth general Inquiry, namely, what it is that fills the derelict space; I now come to the fifth and sixth Inquiry, namely what is not the cause of this suspension of the Mercury, and what seems to me the true cause thereof.

And among those that are assigned for causes of this suspicion, which yet are not causes thereof, I shall mention only these three; namely the renitence or resistance of the Mercury in the restag-  
nant vessel; 2. and the counterpoise of the impendent column of the Atmos-  
phere, equal in base to the base of the Cylinder of Mercury suspended in the Tube; and 3. the Elatery or Spring of the inferior part of the Air.

Of the former briefly in this Chapter, of the two latter more at large in what follows.

*Stevinus* in the Explication of the 10<sup>th</sup> Proposition of his Hydrostaticks, affirms, that the resistence of the base of any fluid Body, is greater in proportion than the *pondus deprimens* of a like fluid:

fluid: which *Fabri* in the sixth of his *Philosophical Dialogues*, pag. 480. and 493, goes about Geometrically to demonstrate: but I confess that it is performed with so much intricacy, and obscurity, that I do not fully apprehend it.

And if this be so, it may seem at first view, that the resistance of the restag-  
nant Mercury in the subjacent vessel,  
which is as it were the base or foot of  
that Mercurial Column of Mercury,  
that is suspended in the Tube, may con-  
tribute something to its sustentation in  
that position.

But upon due consideration I find  
this is but an imagination, and contri-  
butes nothing to this *phænomenon*.

Indeed, if a liquid be impendent upon  
solid base, as Mercury upon Marble; or  
if a lighter or less compacted fluid be bot-  
tomed or imbaſed upon a much more  
gross and compacted fluid, as a Tube full  
of Air upon a Superficies of Water, or a  
Tube full of Water upon a Superficies  
of Mercury, though the Tube be open  
at both ends, the Base will sustain the  
Column of the lighter or less compacted  
fluid.

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But

But if they are of the same kind and consistence, the Base contributes nothing to the sustentation of the impending Column. But if there be no other impediment, it will wholly subside. And therefore if a Tube, open at both ends, be filled with Water, and the lower end stopt with the finger, be immersed under a vessel of Water, and then unstopped; or if an open Tube stopt at the lower end with the finger, be immersed in a vessel of reftagnant Mercury; in the former case the Water, and in the latter the Mercury will wholly subside to the level of the Superficies of the Water or Mercury in the subjacent vessel; which would not be, notwithstanding the Tube were open at both ends, if a Base of the same fluid had any force or resistance against its descent.

Nay the truth is, the position of the Water or Mercury in the Tube, in any the least degree higher than the Superficies of the Water or Mercury in the vessel, hath an accidental Gravitation upon a lower Superficies of the same fluid.

fluid, by reason of such position or elevation of the fluid in the Tube, above the Superficies of the fluid in the vessel, and by that position presseth upon it, as shall be farther illustrated hereafter. And therefore the *pondus deprimens* of the same fluid in such an elevated position, conquers and overmatcheth the *potentia sustinens* of the subjected fluid, though it were an Ocean of Water. I therefore lay aside this Supposition, as contributing nothing to this *phenomenon*, and proceed to the examining of those, that with great pretension and patronage are suggested to be the Cause of this Suspension.

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## CHAP. XI.

*Concerning the supposition of the æquipondium of a pillar of the Air, or Atmosphere, as the Cause sustaining the Mercurial Cylinder.*

**T**HIS latter age, as it produced the *Torricellian Experiment*, so it hath produced a new Systeme in Nature for the Solution of it, namely the Gravitation of the Atmosphere, and an *æquipondium* of a Column thereof to the weight of the Mercurial Cylinder contained in the Tube, whereby it is sustained; a fuller Explication whereof hereafter follows.

And it is admirable to see, how much this new opinion hath obtained among the *Virtuosi* of this latter Age; so that as a new fashion is greedily entertained among the Gallants of the times, and an old fashion despised and contemned; so this new *hypothesis* is as greedily entertained by the great Wits of the times, and

and hath obtained the stile of a *Doctrine* ; and the Old Peripatetical Learning , touching *Vacuum*, and the *Inseparability* of Bodies, exterminated, under the reproach of *decantata fugit*.

Nay the theory is so much idolized, that some late Authors have strangely contended for the dignity of being the first Inventers of it.

And some that have defended the Peripatetical Solution of *fugavacui* , as *shottus*, when he wrot his *mirabilia Hydraulica* and *pneumatica* ; yet whether overwhelmed and astonished with the *Magdeburgh* Experiment and varieties thereof, or whether he was not willing to seem to perceive less than the *Virtuosi*, that had espoused the new Solution , becomes a Convert , and fairly retracts his former Opinion , and gives up the Solution of the *Torrilellian* Experiment to the *equipondium* of the like portion of the Atmosphere, in his *Technica Curiosa* , l. 4. cap. 6. q. 2.

And 'tis pleasant to see what a wonderful progeny , and voluminous Systems of Theories, Conjectaries, and Con-

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Conclusions have been since built upon  
this Supposition! wherein though the  
Authors have shewn much wit, subtil-  
ty, and learning, yet they have lost the  
expence of much time, and invention,  
and pains, if at last when all is done, it  
prove but an imagination.

The Supposition therefore seems to  
be this:

1. That the Atmosphere or lower  
portion of the Air, some say seven, some  
twenty two, others fifty, others an hun-  
dred miles high, hath a considerable  
Gravitation upon its inferior parts upon  
the Earth, and upon all Bodies in this  
lower world.

2. That where a Tube, sealed at  
one end, is filled with Mercury, and  
then inverted into a restagnant vessel of  
Mercury, the Mercury will subside to  
twenty nine Inches and half, and no  
lower, because a column of Atmospher-  
ical Air, of the same base with the ca-  
vity of the Tube, and incumbent upon  
the face of the restagnant Mercury, doth  
just counterpoise such a column of Mer-  
cury of twenty nine Inches and half  
high

high: so that if the Mercurial Cylinder be of that bulk, as to weigh five pound, the Atmospheric Cylinder weighs just as much, and so as one artificial scale being charged with an equal weight with the other, stands in this *æquilibrium*: so in this natural Scale the Atmospheric Cylinder pressing upon the restagnant Mercury with an equal weight, to the 29 Inches and half of Mercury in the Tube, sustains and keeps it up in an *æquilibrium*.

3. But if while the Mercurial Cylinder stands thus suspended, there be any little hole made in the sealed top of the Tube, the weight of the Air now pressing in upon the Mercurial Cylinder, takes away that *æquilibrium* which it had with that external column of Atmosphere, while it was protected from the impendent column of Air by the sealed top of the Glass; and now the Mercury is driven down by the equal weight of the Air, and wholly sublides into the restagnant Mercury; for now the Aereal Cylinder without the Tube, and that within and upon the Tube, have

have the same common weight or Gravitation upon the Mercury. And upon these Suppositions they graft innumerable Conclusions, and among the rest that the Air gravitates upon all Bodies; that the exact proportion of the weight of Air is herelience necessarily deducible; for if a column of Mercury, of half an Inch diameter, and twenty nine Inches and half high, weighs two pound (for instance,) then a column of Air of the same diameter, extending from the restagnant Mercury to the top of the Atmosphere, weighs just as much. And from hence they measure the hight of the Atmosphere, or ponderous Air; namely, Air being about a thousand times lighter than the like bulk of Water, and Water being fourteen times lighter than the like bulk of Mercury, it must follow by the rule of proportion, That if two pound of Mercury give a column of half an Inch diameter, to be twenty nine Inches high; then two pound of Atmosphere must give a column of twenty nine times 14000 Inches high: (I am not curious in the

pro-

proportion, but only shew the method of their inferences:) and abundance of such fine Consequences are built upon this Platform, which were too large to enumerate.

Now as touching this Solution of the Suspension of the Mercury in the Tube, by the *equipondium* of a Cylinder of Air, of the same diameter with the Cylinder of Mercury, I say it seems to me an Imagination, and to be utterly untrue.

I will not repeate what I have formerly said, against the Gravitation of the Air, in the fourth Chapter, which yet I take to be sufficient to evince the untruth of this Supposition: but I will subjoin two or three homely Experiments, which with the application of them will, as I think, detect the untruth and inefficacy of this Solution.

1. I will begin with one offered by the Afferters themselves of this Solution, of a late learned Author, in the 11. Proposition of his *Hydrostaticks*, and the *Scholium* thereupon, which I shall agree to be a true Experiment, but the application

158 Observations touching the  
cation thereof too much distorted, to  
gratifie the *hypothesis*, but in truth and  
reality utterly confounding it.

Take a vessel of restagnant Mercury,  
of seven Inches deep; then take a Glais  
Tube of half an Inch diameter in the  
cavity, and of half an Inch thick in  
each side, sealed at the one end, and of 3  
foot long; whereby the entire Tube  
will be an Inch and a half in its whole  
diameter: Fill it with Mercury, and  
stop it with the finger at the open end,  
and invert and immerse it to the bottom  
of the restagnant Mercury. It will then  
come to paſs, that about twenty nine  
Inches and half, the ordinary altitude  
of the column of Mercury, will be above  
the Superficies of the restagnant Mer-  
cury, and the whole Tube will thereby  
remain full to the top: but the Glais  
being lighter than the Mercury, will be  
impelled up by the restagnant Mercury  
near to the Superficies thereof, where-  
by there will remain about fix Inches of  
the upper end of the Tube empty of the  
Mercury, but still there will be about  
twenty nine Inches and half of the Tube

still

still full of Mercury, namely, the usual station of its Suspension.

I will suppose the twenty nine Inches and half of Mercury, suspended in the Tube, to weigh one pound, and that the Glafs Tube of it self weighs likewise one pound.

I will now suppose, that the upper end of the Tube were fixed by a string to the Beam of a pair of Scales, as in his 319th figure in that book. I say that two pound in the adverse Scale will counterpoise this Tube with the included Mercury, and if any little advantage of weight, suppose an Ounce, were added to the adverse Scale, that Scale charged with two pound and an Ounce, will preponderate it.

And I say no more than what the Author says was determined touching it by the *Royal Society*, whereunto he subscribes, pag. 727. in these words. *Pondus in adversa lance contraponderans aequipollebat suspensi hydrargyri cujuscunque altitudinis, atque simul (quantum conjectando estimabant) suspensi tubi ei parti quæ stagnantis in subjecto vase hydrargyri superficie*

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ciei supereminebat.** Now it is certain  
that if the one pound of Mercury con-  
tributes to this weight, it is not sustain-  
ed by the adjacent external column of  
Air, pressing the Superficies of the re-  
stagnant Mercury; for if so, it is sustain-  
ed by the *equipondium* of that Aereal  
column, and cannot affect the Scale with  
its weight; for it is sustained by ano-  
ther counterpoise, and not by the two  
pound weight of the Scale.

And again it is as certain, that if the  
Mercury do contribute to the counter-  
poise of the Scale, it must by some means  
be as it were fastned to the Scale, (we  
will suppose it to be *Linus* his *funiculus*)  
for otherwise the Glass would only  
weigh as a separate thing from the  
Mercury, which would of it self contri-  
bute (*ex suppositione*) but one pound to  
the weight, which would be necessari-  
ly preponderated by the adverse Scale  
charged with two pound weight.

And therefore it is most clear, if the  
included column of Mercury contribu-  
tes one pound weight to the ballance,  
it is not sustained by the imaginary  
column

column of Air equal in diameter to the Mercurial Cylinder, but by an intrinsic connection or ligament to the Tube, whereby it weighs as one concrete Body with the Glass, which I call the *Funiculus ex subtilibus Mercurialibus efferviis contextus*.

The Author, to avoid this difficulty, therefore tells us, that this thing *primo aspectu incautis nonnullis ad Staticam minus attentis facile imponeret*; and to avoid the imposture, tells us, that all this while it is not the Mercury that contributes to the weight, for that hath another kind of Sustentation by the *equi-pondium* of the forreign Air, but it is another column of Air, that impends upon the upper end of the Glass Tube as its Base, and therefore commensurate to the suspended Mercury in weight. And this is the effect of the Solution, namely, that only the Glass, and the column of Air impendent upon it, makes the counterpoise, and the Mercury contributes nothing to it.

And indeed if the column of Air, charging the Tube, were of an equal

M dia-

162 · Observations touching the diameter with the Mercury, or the cavity of the Tube , the Evasion were handsome and suitable to their *hypothesis*, though untrue: but the instance in question, as it is proposed, quite shatters this Solution, and renders it not only untrue, but unsuitable to their very principles.

For in this instance, the diameter of the column of Mercury is but half an Inch, but the diameter of the whole Tube is an Inch and half, which gives an *area* to the base of the Cylinder of Air impending on it, more than four times bigger than the Base of the Mercurial Cylinder. And therefore if a Cylinder of the Atmosphere of half an Inch diameter , be commensurate to the weight of the Mercurial Cylinder, and consequently weighs a full pound at that instant ; the Aereal Cylinder, commensurate to the upper *area* of the Glass Tube, which is an Inch and half diameter, must necessarily weigh above four pounds, and therefore would not be equiponderated by the two pound weight in the opposite Scale.

This

This instance therefore which is delivered with the memorial of the day, and place of its Exhibition, seems to make more to the disadvantage, than the advantage of this new Philosophy.

But to this Objection there is a Solution offered, namely, that let the Glass be of what thickness it will, That alters not the case; for the Mercury is pressed up by the counterpoise of a Cylinder of Air, answerable to the cavity of the Tube, not to the crassitude of its sides: for Glass being a Body specifically lighter than the Mercury, the Glass it self is sustained by the restignant Mercury, which is its Base, and supports it.

But still this Answer, though it be true, avoids not the Objection. For 1. let us suppose, in steid of a Tube of Glass, a Tube of Gold, which being specifically heavier than so much Mercury, would sink to the bottom, and not be sustained by the Mercury, but must be lifted up, and sustained purely by the weight in the opposite Scale: 2. or suppose the Tube of Glass were charged in its closed end with a greater quantity

of Lead, that, though specifically lighter than Mercury, would by its excess of extrinsical weight, depress the Tube to the bottom of the restagnant Mercury: or 3. though the Tryal were made by a Glafs Tube filled with Water, and inverted into a vessel of Water; for there the Glafs Tube, with the inclosed Cylinder of Water, would subside to the bottom, being altogether in its concrete heavier than the Water, and could not be supported by the restagnant Water, but by the weight in the adverse Scale: in these Instances the difficulty will not be salved.

Therefore I took a Glafs Tube, the cavity whereof was half an Inch diameter, the thickness of the Glafs incircling the cavity  $\frac{1}{6}$  of an Inch; and consequently from outside to outside the diameter of the Tube was  $\frac{1}{2}$  of an Inch; the length of the Tube eighteen Inches, the weight thereof in the Air two ounces  $\frac{1}{4}$  of an Ounce, and being heavier than the like bulk of Water, it would freely subside perpendicularly to the bottom of a vessel of restagnant Water dee-

deeper than 18 Inches, notwithstanding the sustentation it had by the included Air, being empty, and so the Water contributed nothing to the support of the Tube when it came to be weighed, but only the thickness of its *medium*, which yet was not able to support it from sinking.

The Water which the Tube would contain, weighed one Ounce and three quarters, wanting about a Grain or two at most.

The Tube was tied at the closed end with a string to the Scale of a Ballance and filled with Water, and stopping it with the finger, inverted into a vessel of Water 18 Inches deep, or more: and then the Tube with the included Water thus immersed in Water, carefully weighed, the Water still remaining suspended in the Tube, (as it will do as long as it stands so immersed,) and the Events were these:

When the Tube was full six Inches above the Superficies of the Water in the Vessel, it weighed just two Ounces and three quarters of an Ounce, which

M 3 weight

166 Observations touching the weight in the opposite Scale kept it in an *equilibrium*; for now the rest of the Water in the Tube, below the Superficies of the Water in the vessel, weighed nothing at all, because it obtained so far the same common Superficies, and was therefore sustained by the common weight which it had with the Water in the vessel, and only six Inches of Water contributed to the counterpoise, together with the weight of the Glass Tube it self, though refracted by the thickness of the *medium*: so that now these six Inches of suspended Water, and the Tube it self, weighed no more at this position in the Water, than what the Tube alone weighed in the Air.

But when the Tube, with the included column of Water was raised very neer eighteen Inches above the Superficies of the vessel of Water, so that there was as little as could well be to keep the orifice of the Tube under Water, *viz.* about a quarter of an Inch, it then required an addition of very neer one Ounce  $\frac{1}{2}$  in the opposite Scale, to hold the Tube in an *equilibrium*, which equalled

led the true weight of the Tube it self and Water included, when in the free Air, *viz.* two Ounces and three quarters, and one Ounce and three quarters, in all four Ounces and half; because now the Water in the Tube had now in effect its due weight, which it had when divided from the Water in the vessel, and so had its due Gravitation upon the Scale.

Upon this Tryal of it with Water these things are observable.

1. That according to the proportion of the Elevation assigned to Water, it would have been sustained in a Tube of one and thirty foot high, and consequently in this of eighteen Inches the Water had not attained a twentieth part of that height, at which it might in a Tube be kept suspended.

2. That according to these mens Suppositions, the Suspension in the Tube, either of Water or Mercury, is supposed to be by the pressure of a column of the Atmosphere upon the Water or Mercury in the vessel, and thereby the column of Water or Mercury is

166 Observations touching the weight in the opposite Scale kept it in an *æquilibrium*; for now the rest of the Water in the Tube, below the Superficies of the Water in the vessel, weighed nothing at all, because it obtained so far the same common Superficies, and was therefore sustained by the common weight which it had with the Water in the vessel, and only six Inches of Water contributed to the counterpoise, together with the weight of the Glass Tube it self, though refracted by the thickness of the *medium*: so that now these six Inches of suspended Water, and the Tube it self, weighed no more at this position in the Water, than what the Tube alone weighed in the Air.

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by that counterpoise driven up in the Tube. This Tube being but of eighteen Inches long, the forreign Atmospherical pillar still will urge the Water or Mercury to rise to an *equipondium* with it, namely the Mercury to twenty nine Inches, the Water to thirty one foot, or thereabouts.

3. And consequently, according to their Supposition, the Cylinder of Water, in the instance in question, within the Tube, presseth up the Tube, and would lift it up to one and thirty foot high, had the Tube been long enoughe.

4. But as the Atmospherical Cylinder without the Tube presseth up the Water, so the Atmospherical Cylinder incumbent upon the head of the Tube, presseth it down; and so according to those men it is not the Water in the Tube, that gives any counterpoise to the opposite Scale, but the Atmospherical Cylinder, answerable to the cavity of the Tube, that presseth upon the head of the Tube.

5. And that weight of the Atmospherical

spherical Cylinder upon the head of the Tube, is counterpoised by the forreign Cylinder, abating only so much, as the proportion of the Water thrust or kept up in the Tube, amounteth to; which being in the instance in hand about a twentieth part of what the forreign Atmospheric pillar weighs, the Atmospheric pillar incumbent upon the head of the Tube weighs just so much as the Water included in the Tube amounts unto. But upon the whole matter, this fine Theory appears to be but a handsom Imagination. For it is plain, in the instance in hand, the Atmospheric Cylinder, incumbent upon the head of the Tube, is larger than the cavity of the Tube, and consequently more weighty by far (if it have any weight at all) than that forreign Cylinder of Atmosphere, which is supposed to sustain the included column of Water, and the Tube it self is not sustained nor sustainable by the Water in the vessel, but is wholly raised by the weight in the opposite Scale: and therefore the Supposition, that the weight that pres-  
feth

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seth upon the Scale, is the weight of  
the Cylinder of Air upon the Tube it  
self, is vain; for then it should sustain  
the weight, not only of the Tube, but  
another weight far greater than the  
forreign column of Air could counter-  
poise.

And that which seems to me to con-  
vince this mistake in the whole propo-  
sed Theory, is this plain and obvious  
Experiment.

*Fig. 10.* I therefore took a Glass-tube of  
about half an Inch diameter, 18 Inches  
long, closed at one end; and weighing  
it exactly in the open Air, it weighed  
two Ounces and three quarters of an  
Ounce, and heating it very hot, where-  
by the included Air was highly rarified,  
I suspended the closed end upon one  
Scale of a Billance, and let the open end  
sink a little into a vessel of Water, and  
counterpoised it in the other Scale with  
two Ounces three quarters; and be-  
cause the end of the Tube did but little  
more than touch the Water, it held the  
same weight as before upon the Scale,  
viz, two Ounces three quarters.

As

As soon as the Tube was thus continuoous to the Water, it attracted and raised the Water so, that in half a quarter of an hour the Tube was filled twelve Inches with Water, the Air contracting it self to six Inches in the top of the Tube, whereby it appears it was rarified two thirds beyond its natural and ordinary rise and standard, which now againe it acquired by contracting it self.

The Water which it attracted being carefully marked at its highest Elevation, and then strictly measured, weighed one Ounce and a quarter.

Now the things observable in this instance are these. 1. That the single Tube, with the included rarified Air, did really weigh even in the external Air but two Ounces three quarters, and by that weight in the opposite Scale it was perfectly kept *in aequilibrio*. 2. That when it touched the Superficies of the Water, and thereby attracted the Water into it, it presently grew more weighty, which it gradually increased, and accordingly more and more subsided, Tube and all, into the subiectet vessel.

172 Observations touching the  
vessel of Water, as it received into it  
more Water, and consequently grew  
more weighty, 'till its full comple-  
ment of 12 Inches of Water drawn up  
into it. 3. That when it was filled with  
twelve Inches of Water, then it weigh-  
ed the full weight of the Tube, and  
the Water, *viz.* two Ounces three quar-  
ters for the weight of the Tube, and  
one Ounce and a quarter for the weight  
of the Water *in toto* four Ounces, and  
therefore four Ounces in the opposite  
Scale would keep it just *in aequibrio*. Only  
care must be taken, either by depre-  
ssing the Water, or raising the Ballance,  
that the Tube must only touch the top  
of the Water, for otherwise the Water  
will run out, and prevent the tryal.  
Now to discover what it is that thus de-  
presseth the Tube gradually, as the  
Water riseth up into the Tube, whe-  
ther the weight of the Tube, or the  
weight of the Water included in it, I  
will consider,

1. What the external column of  
Air, which those Masters suppose to  
press up the Water in the Tube, contri-  
butes

butes to it. and surely according to this Supposition of theirs, this is so far from causing any access of Gravitation to the Tube or Water included in it, that it rather presseth up the Tube, by pressing and impelling the Water into it; and indeed it is that which they themselves say, and build their Theories upon it, *viz.* that for this reason the Water in the Tube weighs nothing.

2. What the column of Air impendent upon the top of the Tube, commensurate in diameter to the cavity of the Tube, doth contribute, which according to their Solution must be it that weighs down the Tube by pressing upon it. But certainly this doth nothing in this case; for if the Tube it self, filled with rarified Air, weighs in the open Air but two Ounces three quarters, it can never come to weigh four Ounces when in the Water, (where by reason of the thickness of the *medium*, it must lose, and not gain weight,) and it had the same column of Air impendent upon its top, as well before its immersion in the Water, as after. And before its im-

174 Observations touching the immersion (whether it were full of common Air, or of Air thus highly rarified by heat) it still weighed neither more nor less than two Ounces three quarters.

3. Therefore it remains, that it is the weight of the Water, that thus creeps up in the Tube, and in its ascent to twelve Inches weighed just one Ounce and a quarter that gravitates in the Tube, and renders the weight of the whole to be four Ounces, which in the opposite Scale holds both Tube and Water *in æquilibrio*.

And as the Tube is suspended to the Ballance by the string or thrid, so the included rarified Air is suspended to the top of the cavity of the Tube, by the powerful Laws of Nature, and lays hold upon the Superficies of the Water; and as it contracts it self more and more, draws up the column of Water higher and higher; as a Lute-string fastned to a fixed Body above, and to a small Weight below, after a high tension raiseth the Weight by contracting it self, and this to maintain the continuity of the parts of Nature.

And

And this is a plain sensible and unforced Solution ; and it seems to me , that those that tell us , that it is only the Tube, and the incumbent Cylinder of Air only, that gives the weight , were imposed upon, though not by ignorance of Statical Laws , yet by fondness and partiality to a new invented *hypothesis*.

2. The second Instance that I shall give, shall be this : Take a Glass Tube, closed at one end, of an Inch diameter in the cavity ; fill it with Mercury, stop it with a small Engine, and invert it into a vessel of restagnant Mercury of an Inch and half diameter ; then opening it by a contrivance which is not difficult, after it is inverted in the Mercury, the Mercury will be sustained at twenty nine Inches and half , as well as if the vessel of restagnant Mercury were of six Inches diameter.

And yet it is apparent , that the rim or round Superficies of the vessel of restagnant Mercury, is not above one fourth part in *area* of the Mercurial Cylinder, and cannot be pressed with a column of Air commensurate to the cavity

174 Observations touching the  
immersion (whether it were full of com-  
mon Air, or of Air thus highly rarified  
by heat) it still weighed neither more

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And yet it is apparent , that the rim or round Superficies of the vessel of restagnant Mercury, is not above one fourth part in *area* of the Mercurial Cylinder, and cannot be pressed with a column of Air cominensurate to the cavity

176 **Observations** touching the  
ty of the Tube; for the incircling co-  
lumn of Mercury is not a fourth part of  
the *area* of the Tube, neither can it be  
pressed with a column of Air of a grea-  
ter diameter or dimension than that li-  
tle rim of Mercury that incompassed  
the Tube. So that it is not the *equipon-  
dium* of the Atmospheric Column  
that sustains the Mercury in the Tube,  
for if it should, a column of Air of the  
fourth part of an Inch diameter, should  
be as weighty as a column of Air of an  
Inch diameter, because it must in this  
Instance make an *equipondium* to a  
weight of Mercury, commensurate to a  
column of Air of an Inch diameter.

Therefore it is not the *equipondium* of  
the Air that sustains the Mercury in  
the Tube, but something else, and what  
that is, we shall hereafter examine.

3. My third Instance is that com-  
mon one: If the *Torriceilian* Engine be  
put in practice, in a great Glass Recei-  
ver, wholly excluding any communi-  
cation with the forreign Air, the Mer-  
cury will be suspended at twenty nine  
Inches and half, as well as in the free  
Air.

Air. And yet here can be no Atomo-spherical column, reaching from the top of the Atmosphere to the stagnat Mercury, with its Gravitation, to sustain the Mercurial column, the same being intercepted and broken by the interposition of the Glafs.

And this plain obvious evident Instance had wholly destroyed the *hypothesis*, and utterly disordered and scattered all the *porismata* and Consequences built upon it, had not another supplemental invention stept in to relieve it, namely the prodigious Elatery of the free Air, which upon a due examination will prove as great a mistake, and as impotent to relieve this deficient *hypothesis* as the former ; and if it prove so, I doubt our new Philosophers must be fain to return to the *decentata fuga vacui* for a better Solution.

## CHAP. XII.

## Touching the Elatery or Spring of the Air.

**I** Come now to consider that subsidiary help, substituted by the New Philosophy, for the Solution of the *phenomena* of the *Torriceilian* and other Experiments of this nature: and herein I shall consider these things.

1. What Elatery or Spring is of all hands agreed to be truly found in the Air.

2. What Elatery or Spring is substituted by the late Philosophy, and how applied by them to the Solution of *phenomena* of this kind.

3. The Arguments, Reasons, and Evidences that evince the mistake of this new Elaterical Supposition.

First, as to the former of these, this is agreed of all hands, that the Air being a subtle Fluid, is capable of an accidental Elasticity of very great efficacy and force: namely, when by accidental impression

pression upon it, it loseth that common and natural state or consistence, that otherwise belongs to it.

As 1. by a great compression of its parts together, such as we see in Wind-guns, whereby it is compressed into a space sometimes seven times less than that which is natural to it, and then by a natural and strange motion of restitution it hath an Elasticity, whereby to regain its natural state and position. 2. By constipation of its parts by Cold, whereby it is bound up sometimes to a narrower room, and then by the approach of convenient heat, that constipation is relaxed, and the Air regains its natural liberty, motion, and consistency: and 3. by Rarification by heat, whereby it is expanded beyond its natural size, and as it grows cold, it obtains a kind of Elasticity inward, and an inward endeavouring to contract it self to its natural size and dimension. 4. In a strong tension by an accidental or collateral force, whereupon it contracts it self as much as it can to its natural state, and resists that tension

N 2 which

which discomposeth and extends it; from whence follows that which is usually called Suction, or Attraction, of which I shall have occasion hereafter to write more at large.

Again 2. it must be agreed, that although the Air in its natural consistence is by a great force compressible into a close order, yet it doth resist such a compression, and doth not easily yeild to it; and therefore take a Tube of any considerable length, closed at one end, and the open end inverted upon the Water, and pressed down, yet the Air included will retain very much of its natural state, and receives but a small contraction by the counterpressure of the Water, possibly an Inch in two or three, or somewhat more, according to the depth of its immersion.

But these Elateries are not such as concern the present question.

Therefore I come to the Supposition of the new Philosophy, in relation to this business of Elasticity of the Air, which they attribute to the common and free Air in which we breath and live;

live ; and if I mistake them not, their Supposition is this :

1. That the Air is a kind of Spungy Body, not much unlike (as to this purpose) to a Spunge or Fleece of Wool, capable of compression, and under that compression endeavouring to loosen and discharge it self by that which we call Springiness or Elasticity.

And herein we do not considerably differ, only they say that this Elasticity is of a vast, and almost unlimited nature, that scarce hath any bounds to it ; we say that it hath more contracted and determined limits of its Elasticity, and those are such as every portion of free Air obtains in a common and usual state of the temperament of the Weather.

2. Again they say, that the Body of the Air, or at least of the Atmosphere, consists of a great many little crooked filaments like Springes, which when extended to a greater straightnes than is suitable to them, as in its high tension or rarification, indeavour regaining of their natural curvitude ; and when by any weight or force they are drawn close to-

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gether, they spring out to their just ex-  
tension, and that gives a common  
Springiness to the Air in such a confi-  
stence or circumstance.

3. That the superior parts of the  
Air gradually gravitate and compress  
the inferior parts, and therefore the  
higher the Atmosphere or Air is, the  
less it is compressed; but the lower it is,  
the more it is compressed, the lower-  
most parts receiving and sustaining the  
pressure of all these parts that are above.

4. That the actual Elatery, or  
Spring of the Atmosphere, or Air, is  
the issue or effect of this Gravitation;  
and the more the Gravitation is, the  
greater the Spring of those parts are,  
when they can but get liberty to expand  
themselves; for the greater weight  
doth the more bind and incurvate these  
crooked filaments, which makes them  
fly out extremely, when they can get  
themselves unfettered from the oppres-  
sing weight, and even while they are  
under that pressure they have their *cons-  
tus* for their liberty.

5. That because the lower parts of  
the

the Air next the Earth are most pressed upon by the superior parts, therefore the lower parts of the Air have the greatest Elasticity and Springiness, which it exerciseth according as it gets liberty.

6. That therefore this inferior Air hath a natural vast propensity to Extension, and amplifying of it self, insomuch that if we should suppose any part of the Universe were free from any Body, or had a more subtle Body than that of the common Air, if a portion of the lower Air next the Earth were placed there, it would upon the account of its own natural Elasticity, expand it self as largely, and it may be much larger than those Artificial Expansions wrought by fire in *Æolipiles*, and other Engines, wherein nevertheless the Air may be so ordered, as to possess seventy times more room than before it had.

7. That therefore when we see a little portion of Air included in a Tube, in the *Torriceilian* Experiment, extended upon the descent of the Mercury four, five, or six Inches, or into a larger room than it possessed upon its first in-

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clusion, this is no forced tension of the  
Air, but a free natural expansion of it  
self, having gained room and opportu-  
nity, by the descent of the Mercury, for  
its reception ; and therefore this natu-  
ral expansion of that included particle  
of Air, is one great help towards the de-  
scent of the Mercury, 'till it comes to  
its station.

8. That in as much as the Elasticity  
of the Air, when contiguous to the  
Earth, or other solid Body, is impeded  
in its Elasticity downward by the resi-  
stance of the Earth, it is as it were re-  
verberated upward, and laterally, where  
it may be exercised : which is a great  
means of that support that the recoiling  
Aereal particles give to weighty Bodies,  
which is seen in the sustentation of the  
lower of the adhering Marbles.

9. That every part of the lower Air  
is under the same compression with the  
same region wherein it is, and therefore  
if a portion of the free or open Air be  
inclosed within a Glafs-Receiver, closed  
on every side, yet it hath the same Elas-  
ticity that the portion of Air had, out  
of which it was taken.      10.

10. And because that Air in the Glass is on every side penned up, and its Elasticity being as it were reverberated from every side of the Glass, doth the more intensely and forcibly recoil inward: and therefore it comes to pass, that though the great Glass Receiver, compassing in the *Torricellian* Engine on every side, be destitute of the weight of the column of Atmosphere, to suspend the Mercury by its *equi pondum*; yet the included Air doth the very same thing by its Elasticity or Spring upon the stagnant Mercury.

And thus we see how, for the accommodating of the *Torricellian phænomena*, and some others of like nature, where the *pondus Atmosphæricum* cannot be had nor acquired, what a laborious invention and process is framed for the supply of that deficiency.

Therefore I come to consider and examine, whether this laborious process have any thing of truth and reality in it, or whether, as that of the *pondus cylindri Atmosphærici*, it be only a fine invention to help and support a lame and lan-

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languishing *hypothesis* or Solution.

Although whatsoever doth oppose the Gravitation of the common Air, doth as necessarily conclude against this Elasticity of the free and common Air; and although much of what I have written in the third and tenth Chapters, against the Gravitation of the Air, is equally applicable against this imaginary Elasticity thereof, since the Elasticity is by the *hypothesis* supposed to be the effect of its Gravitation; yet I shall repeate nothing, or very little, of what I have there said, but apply my self singly to the discussion of this Elatery or Springines of the common or free Air, both by Reason and by Experiments.

1. The Spring of the Air is bottomed upon the supposition of the actual pressure of the superior parts of the Air or Atmosphere upon the inferior, and the lower parts being under the greater pressure, hath consequently the greater Elatery. But upon what hath been before said, in the third Chapter, there is no such Gravitation or pressure of the Air or Atmosphere, and consequently

no such Elatery in the free and open Air.

2. If there should be admitted any such Elasticity in a portion of the Air below in this inferior region, included within a Glass vessel or Receiver, (the instance for whose sake this invention was principally devised,) so as to sustain the suspended column of Mercury, then of necessity there must be the same Elasticity of the same or the like portion of the common Air, (for it is all supposed under the same common pressure of the superior parts:) the consequence whereof must needs be, that either the attribution of that Suspension to the Gravitation of a Column of the Air, and all the delicate Consectaries thereupon, are more Fancies and Mistakes, because the whole *phenomenon* is performable and performed by the Elasticity of the Air, as well without as within this close Glass vessel; or else the joint power of the *pondus*, as well as the Elasticity of the forreign Air, must raise the Mercury in the Tube to fifty nine Inches, since the Elatery of the Air alone, without the

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the aid of the Gravity of the Atmosph-  
rical Cylinder in the inclosed vessel, is  
able to keep it suspended to twenty nine  
Inches and half. But we see the con-  
trary in the Experiment tried, as well  
within as without the inclosed Glass;  
therefore either the weight of the At-  
mospherical Cylinder, or its Elatery,  
must be laid aside in this Solution, or  
which is the truer Conclusion, neither  
the one nor the other are to be admitted,  
but both are to be rejected as crossing  
one another, and also the truth of the  
Solution.

3. Nay, but yet farther, if this El-  
atery be admitted, the whole Scheme of  
the Gravitation of the Air, or any part  
thereof, must be rejected as fictitious, and  
impossible; and on the other side, if the  
supposed Gravitation of the Air or At-  
mospherical Cylinder be admitted, the  
Elatery must be rejected, and the whole  
Air will be *in equilibrio*, in as much as  
upon this supposition of this New Phi-  
losophy, the Elatery presseth as much  
upward, as the Gravitation doth down-  
ward, and *è converso* the Gravitation as  
much

much downward, as the Elatery doth upward: so that after all this laborious apparatus of Gravitation, and Cylinders, and Atmosphere, and Elatery, the Air is at quiet, at rest, and *in æquilibrio*, which is easily granted without all this difficult *hypothesis*.

For according to these mens supposition (if we should admit it) the Atmosphere were seven Miles high, we must needs suppose that the greatest Elatery is next the Superficies of the Earth, because there it is more charged with the weight of the superior parts of the Air; and because the terrestrial Globe checks the Elatery of it downward; the Elatery must discharge it self by its motion upward, and so encounter the pressure of the Air, by how much more its Springs and crooked Particles are charged. And a Mile higher in the Atmosphere where is less pressure, there is consequently less Elatery or resistance of that pressure, and yet sufficient to keep that rundle of Atmosphere *in an æquilibrium*.

And it is plain, that this Counter-motion of the supposed Spring, or Elatery,

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ter, and Gravitation of the Air, must  
by their mutual conflict, one check and  
refract the other.

But then it is hoped, that the interpo-  
sition of another gross Body between  
the ponderous Air tending downward,  
and the reverberated, recoiling, Elasti-  
cal Air tending upward, may part the  
fray; and thereupon in the instance of  
the two cohering Marbles, the upper  
column of impending Air may press  
down the upper Marble, and the lower  
column of recoiling Air may press up  
the lower Marble.

But this will do nothing to help the  
business in hand, touching the Mer-  
curial column in the *Torriceillian* Experi-  
ment, (for as for the cohesion of the two  
Marbles, and the reason of it, I shall in  
its due place examine.)

For it is plain there, that if there be  
any Elasticity in the Air next the Earth,  
there is proportion for proportion as  
much Elasticity in the Air next the re-  
stagnant Mercury, upon which it is in-  
cumbent, and which we may if we  
please, place even to the Superficies of

the Earth; for the column of Air (*ex suppositione*) proportionable in base to the Mercury, is as really gravitating upon the Mercury, as upon the Earth, and consequently there is the very same Elatery or *sursum* pressure of inferior parts of that Atmospheric pillar incumbent upon the Mercury, that would break the Gravitation of that upper Atmospheric Cylinder, and render it ineffectual.

Besides all this, as it requires a strong Phantasy to suppose this wonderful Elasticity of the Air, so it requires a much more potent phantasy to keep the Elasticity of it within the precise lateral dimensions of the Atmospheric pillar in a straight line: for the Elastical particles of the Aereal parts, adjacent to that Atmospheric Cylinder, will incroach and break it upon this Cylinder, and disorder and refract its pressure, besides what the concussion of Winds would effect.

4. But suppose in the free Air the *pondus deprimens* of the Atmospheric Cylinder were too hard for its Elatery, and

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so although in an inclosed Glass the Elasticity doth the whole business in suspending the Mercury in the Tube, yet in the open Air the *pondus deprimens* conquers its Elasticity, and the whole *phenomenon* shall be there performed by the *pondus deprimens* of the aereal Cylinder: I say such a Supposition as this would render the lower parts of the Air unmoveable, and bind it up so, that it could never stir without an admirable relaxation of it by the violence of heat, which is contrary to all the experience of the World. For we see by the smallest winds the motion of Smoak, and a thousand Instances more, the lower parts of the Air are in a perpetual flux and motion, which it could never do, if under so rigorous a constipation and compression by its superior parts. And though this immediately contradicts the great pressure of the inferior Air by the superior, yet by necessary consequence it contradicts the supposed Elasticity thereof.

5. But as to the very instance before given, of the suspension of the Mercury

in

in a Tube placed in a closed Glass Receiver, protected from the impendent aereal ponderous Cylinder; I say, that the suspension of the Mercury is not caused either by the Weight or the Elatery of the Air. Not by its weight, because the Atmospheric column is intercepted, and kept off by the interposition of the Glass; it remains therefore that it must be done by the Elatery or Spring of the Air included in the vessel: but that cannot possibly do it, for these Reasons. 1. Because that Elatery that must sustain the column of Mercury, cannot be an Elatery pressing upward or laterally, but it must be such an Elatery, as must press downward, to bear against the excursion of the suspended column of Mercury, and thereby keep it *in aequilibrio*: and certainly if there be any Elatery of the Air, it must bear rather upward against what presseth it down, and cauleth it, than downward. 2. But if it be said that by the Receiver, or Glass inclosing the Air every way, the Elatery is as well turned downward as upward, and so counterpoiseth the co-

O luma

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lumn of Mercury, and so it is not like the recoil of the Spring of the Air from the Earth, which must be principally upward, because checked and reverberated at its base; but here the reper-  
cussion is from all parts of the Glass, as well from above as beneath. I say this very objection renders it utterly impos-  
sible, that the Elatery of the Air in the Glass can cause this Suspension, or in any measure contribute to it. For let us suppose the Glass Receiver to be spher-  
ical, the repercussion of the Elatery of the Air must be equal from all parts: the consequence whereof must necessa-  
rily be, that this Elatery presseth as well upwards as downward, and from every part of the Spherical cavity of the Glass, the consequence whereof must needs be, that the whole Elatery is in suspence, and one Elastical line breaks, refracts, and retunds another; so that the tendency of this Elasticity is neither upwards nor downwards, nor from any point of the Compass, but all things are at rest, as where Winds blow with an equal strength every way, the

Air

Air is undisturbed, and so no pressure at all made upon the Mercury, but it is but a meer evasion to shore up an *hypothesis*.

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### CHAP. XIII.

*Certain Experiments, evincing that the common Air in its ordinary consistence, hath no, or no considerable Elasticity or Spring, without an accidental or preternatural Compression.*

IN the former Chapter I have given the reasons against the wonderful Elasticity of the Air, or Atmosphere, and the insufficiency of that Supposition to give a Solution to the *phenomena* in the *Torricellian Experiment*. And I now come to some experimental Instances, farther illustrating and proving what I have said in the former Chapter, namely that either there is no Elasticity at all in the common Air, nor in any part thereof inclosed in the vessel, *de quo supra*, or if any at all, yet very lit-

tle, and such as is no ways able to keep such a column of Mercury suspended, which may possibly weigh according to the amplitude of the Tube two or three pounds, which yet we see suspended in the *Torrilellian* Experiment.

For it is a certain and demonstrable truth, that if any *potentia*, be it what it will, sustains a weight or *pondus deprimens*, the *potentia sustentans* must be of an equal strength to that weight, otherwise it must give way, and yeild to it.

Therefore if the *potentia* of the Elasticity of the common Air (not compressed or constipated accidentally) be not equal in this instance to two pound of Mercury, it can never sustain or keep it suspended: and this is that which I shall endeavour to make good.

*Fig. 12.* Take a Bottle-Glass *A*, fill it with Water to *B*, which may contain about six Ounces of Water, the rest of the Bottle full of Air; then take the Bolts-head *C*, heat it very hot, that the Air may be in a great measure driven out

out of it, and the rest highly rarified and expanded: Invert it into the Glass Bottle, so that the free Air may come in, (though but at a Pin-hole,) as the Air in the Bolts-head grows cool, and consequently contracted neer to its natural size and state: the Air thus rarified, the Water in the Bottle will be drawn up into the Bolts-head to supply the vacancy, which otherwise would happen by the restitution of the Air. But if the fissure between the Bottle and Shank of the Bolts-head had been hastily so closed, that no Air could enter into it, some Water would arise as suppose to *D*, but the whole Water would never be driven up out of the Bottle by any Elatery of the Air included in it; but only the superior Air in *C*, being so highly rarified by the heat, that one third part of the Air in *C*, as it stood unrarified, takes up the whole Bolts-head by rarification; and as the Air in the Bolts-head grows to its natural temper, and consequently endeavours to retract it self to its natural dimension, so having the fluid Water to work upon, it draws it up, and by

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the advantage of that attraction gives a tension or expansion to the Air in the upper part of the Bottle, as much as is capable to be obtained by the strength of the attraction and suction of the rarified Air in the Bolts-head, which drawing up the Water, and leaving les Water there than before, must needs expand the inclosed Air in the top of the Bottle, to supply that recess.

Now if the Air in the head of the Bottle, being taken out of the common Air, and of the same consistence with it, had that strong Elatery that these men suppose, so that it reacheth as it were after a dimension five, nay forty times larger than that it hath ; it would thrust up the whole Water in the Bottle as high as *E*, as it would have done if the fissure between the Bolts-head and the Bottle had not been strictly closed.

And this it should have done the rather, because the superior Air is so much rarified, that it takes up three times the room of common unexpanded Air, and so is capable of a more effectual impression from the Air in the Bottle, and would

would not rest at *D*, but drive up the Water as high as *E*.

And if any shall say, that the weight of the interposed Water is too great for its Elatery, to thrust it up as high as *E*; certainly he that shall suppose that the Elatery of the Air, included in a cloie Receiver, is able to thrust and keep a column of Mercury to twenty nine Inches high, which column weighs possibly a pound or more, cannot doubt but the Elatery of the Air in the top of the Bottle, if it were of that force that these men suppose, would thrust up five or six Ounces of Water, about five or six Inches high, into the Bolts-head.

But the plain truth is, that in this instance, nor any of the like nature, it is neither the weight nor the Elatery of the Air included in the Bottle, that forceth up any drop of Water above its Superficies, but the powerful attraction of the expanded Air in the Bolts-head, endeavouring its own contraction when growing cold, and so attracting the Water to supply that *casma*, that would otherwise follow upon its attraction.

O 4 And

And therefore when by the descent of the Mercury a little portion of Air in the top of the Tube in the *Torriceilian Engin*, gains four or five times a greater extent than it had before; this is done by the tension of that little aereal Cylinder, by the force of the descent of the Mercury, as shall be more at large shewn hereafter, and not as is groundlessly supposed by the natural Elasticity or expansion of that portion of Air. For if that should be the cause, it were necessary that the finger stopping the top of the inverted Tube open at both ends, should be thrust up, and not drawn in, and an empty Bladder should be more compressed than distended by such Elasticity of the included Air, the contrary whereof is evident both to Sense and Experience.

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## CHAP. XIV.

*The consideration of some Arguments for the weight and Elatery of the Air.*

I Shall not spend much time upon These arguments, but only note some general Observations upon them, and then single out some Instances wherein I shall be more particular.

In general therefore I say,

1. That all those Instances and Experiments produced to prove such a Weight or Elasticity of the free Air, that may be capable of effecting the *phenomena* in the *Torriceilian* Experiment, yea or in the *Magdeburgh* Instrument, or the *English* Air-pump, are all capable of another Solution, as appears by the answers that *Linus* hath given to them, which I need not repeat.

2. That the application of the Weight and Elasticity of the Air to the Solution of these *phenomena*, is forced, strained, and in many instances contrary

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ry to the evidence of Sense; so that  
though the appearances themselves are  
true, yet those Causes assignd are very  
inevident, full of difficulties and ab-  
surdities, and require such a train of supple-  
mental Suppositions to make them but  
tolerably applicable, that no man dis-  
ingaged from a party can easily subscribe  
to them.

3. That there are some Instances  
and Experiments that are so untractable  
to this method of Solution, that all the  
inventions of the most Mercurial wits  
and highest ingaged in the patronage  
thereof, cannot by any means relieve it.

4. That the plain and common So-  
lution of them by the inseparability of  
Bodies, and the *fuga vacui*, (pleasantly  
by some stiled *decentata olim fuga vacui*)  
doth most naturally, and without any  
stress offered to Sense or Nature, answer  
all Instances and difficulties, and fails  
in none.

5. That it is most evident to any  
man, that without prejudice or partiali-  
ty shall observe it, all or the most part  
of those Instances and Experiments,  
which

which (to serve the *hypothesis* of the gravitation and Elasticity of the common Air) are resolved into Pression or Elasticity of the Air, are plainly performed by Suction and Attraction of the Air, when put under a great tension or rarefaction, which wholly subverts the Solution by Pressure or Elasticity of the Air.

Now as to some particular Instances.

First, we are told that the *Torriceilian* Engin, wherein at the bottom of the hill, the Mercury riseth to twenty nine Inches and half, carried up to the top of the hill *Puy de dome* in *France*, subfides three Inches: and some Instances of like nature given in *Lancashire*, it subfides neer two Inches. From whence it is inferred, that this variation ariseth, because upon the top of the Mountain, which was 3000 foot high, the column of the Atmosphere was shorter, and so gravitated less upon the restagnant Mercury, than it did at the bottom of the hill, which caused the disparity of the position.

I answer, admitting the fact true, yet the

the cause assigned is *non causa pro causa*, or at least we have no sufficient evidence that this is the cause.

The various Elevation or depression of the Mercury is not in respect of the height or lowness of the place, where the Instrument is used, but in respect of the temperament of the Air wherein it is used. The Excellent Author tells us, in his eighteenth Experiment, that the *Torriceilian Engin* placed in his Chamber window for a good season, the Mercury had various Elevations and subsidings, according to the temper of the Air; and that this variation was considerable, namely two Inches, and that in cold weather, contrary to the use of other Weather-Glasses, it fell much lower than at other times. And if this be so, it is no wonder, at the top of the hill, where the Air was much colder, it might subside considerably from the height it had at the bottom.

But whether it were colder or hotter, yet certainly there is that difference of temperament in the various regions of the Air, that might make a various position

position in the Mercury. What if we should say, that the *vis ignea penetrativa Aetheris* is stronger there than below the hill, and so might strongly dilate that small portion of Air upon the top of the hill, though the sense of heat be there less?

But I cannot chuse but observe, that if we should assign this Subsidence of three Inches of Mercury to the shortning of the Atmospherical Cylinder, by 500 Fathoms of perpendicular height, we must necessarily conclude the great mistakes in the computation of the gravitable Air or Atmosphere, and must render it of a prodigious height beyond all the Estimates of the latter Elaterists, whereof some tell us it is seven Miles high. By others Computation that it is 33893 Feet, which is 5648 Fathoms, somewhat less than seven Miles.

But if a Mile next the Earth should abate but three Inches in the height of the Mercury, six Miles more would hardly abate three Inches more, *viz.* six Inches; nor twenty four Miles more would

would abate nine Inches. For a Mile of Air next the Earth (if the gravitation of the Air should be admitted) would more than ten times counterpoise the next Mile about it, because it hath these advantages, 1. of the greater incumbent weight of the superior Air, 2. of the stubborn resistance of the terrestrial Globe, whereby it is more constipated and bound together the nearer it is to its reflexion and repercussion. 3. The great concrement and accession that it hath of grosser vapors, and terrestrial effluxes, wherewith the lower parts must needs be more laden than the upper; so that from the first gravitating particle of Air or Atmosphere to the lowest, it will increase by a Geometrical Progression in every part, as 1, 2, 4, 8, 16, or very near it, which will render the lowest Mile prodigiously more weighty than any of the superior Miles.

But I dismiss this busines of the gravitation of the free Air as a meer Notion.

The Instances to prove the Elasticity of the Air, are for the most part such as

are endeavoured to be made out by the tension or rarification of the Air by the Wind-pump, or *Magdeburgh* Instrument. And the answer to any one of them gives an answer to all, for the application of them to the Elasticity or Spring of the included Air is bottomed upon one common mistaken ground. I shall therefore mention but one or two of them.

The Excellent Author in his Book of *Physico-Mechanical Experiments*, *Exp. 5.* gives an account, how a dry Bladder well tied, and blown moderately full, and fastned into the inside of the Receiver in the Air-pump, upon a strong exhaustion of the Pump was broken; and in the 16<sup>th</sup> Experiment, a Glass included in the Receiver, broken by the exhausting of the Air. And in the Continuation of new Experiments, *Exper. 7* and *9*, the same Instance of breaking of included Bladders and Glasses is repeated, and attributed always to the natural dilatation of the internal Air included in the Glass and Bladder, the circumjacent Air being rarified by tension.

But

But the true Solution of both is quite another matter. The Bladder is dilated by the attraction wrought by the tensed or expanded Air, upon the outside of the Bladder, and thereby the included Air necessarily expanded by that force, stretching out the sides of the Bladder that includes it, and when it is capable of no greater stretching it breaks, not by the natural expansion of the included Air, but by the strong tension wrought upon the Bladder by the attraction of rarified or tensed Air in the Receiver, that tears the Bladder.

And as to the Sealed Glass-bubble, the fracture thereof with that violence is not by the natural Elasticity of the Air included in it, but by the strong attraction wrought upon every part of its external Superficies, by the tension of the strongly circumjacent tensed Air in the Receiver.

And therefore if the Bladder or Bubble were filled with Water, and closed fast, (which is not capable of any natural Elasticity) I make little question but the strong attraction of the expanded Air

Air in the Receiver would break the Glass and Bladder, especially if it have any angles, (though I confess I cannot be very confident of what I never tried.)

And yet if it should succeed according as I suppose, though the natural Elasticity of included Air would be thereby convinced of untruth; I expect there must be another expedient invented by the Elaterists to salve it, *viz.* as prodigious a weight in the included Water, as there is supposed an Elasticity in the included Air.

In the late Book intituled, *New Experiments about the Pressure and Spring of Air upon Bodies under Water*, Experiment the second, I find this Experiment, which is to the effect following.

Take a Bladder, and squeezing out the greatest part of its Air, tye the neck very close, and fastning a competent Weight to it, place it in a great large Glass, and placing that Glass upon the Air-pump, fill it with Water, and then covering the Glass with a large Receiver well luted to the Pump, the Air pumped out of the large Receiver, the

P Bladder

Bladder below the Water would gradually swell, 'till by often pumping out the Air, the Bladder would be full blown. And the Supposition is this, that the Air being highly rarified and extended in the Receiver, whereby the strength of its counterpressure or Spring was weakned and abated; the little portion of Air in the Bladder, notwithstanding the interposition of the Water, having the advantage thereby of expanding it self, did accordingly fill up the Bladder.

The truth of the fact is unquestionable, but the Solution of it by the natural Spring of the Air in the Bladder, is an assignation of *non causa pro causa*; but the true Cause is, that by the strong tension of the Air in the Receiver, it lays hold and attracts upon all the parts of the Water, and all that is included in it, and the Bladder being within the reach of that forcible attraction, hath its sides thereby distended, and the included portion of Air expanded and rarified by the tension upon the Bladder; not by any natural Elasticity of the included

cluded Air, but by the forcible attraction wrought upon the external Superficies of the Bladder, and pulling it asunder, whereby the included Air by the common Law of Nature is necessarily expanded to prevent a vacuity.

And as if there had been no Water interposed, the Bladder would thus have been extended, and the Air included in it expanded, by the strong attraction of the rarified Air in the Receiver; so the same effect will as necessarily ensue, notwithstanding the interposition of the body of Water: for that *vis attractiva* of the expanded Air in the Receiver, though it immediately fasten upon the Superficies of the Water in the vessel, yet thereby and therewith it also pervades the whole body of the Water, which being a fluid body hath thereby a tension made upon it, and all its parts, and all that is included in it.

And the truth of this appears beyond dispute, for the Learned Author in his 19, 21, 22, 23, &c Experiments, tells us, and that truly, that by this pumping and rarifying of the Air in the great Re-

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ceiver, Water, Spirit of Wine, &c. will be raised to run out of a Glass included in it, & that bubbles will thereby be formed at the bottom of the included vessel of Water: whereof the true cause (whatever be otherwise assigned) is the forcible attraction of the tensed and expanded Air in the Receiver upon the included Water, which strongly pervades the whole body of Water, penetrates to the very fund or bottom of it, and puts it altogether into a various agitation.

And therefore as this attraction pervades the whole body of water, so it peringeth to the included Bladder in the very same manner as it doth upon the Water, though with a more sensible extenuescence of the Bladder, because more capable of it.

We need not resort to a more evident Instance than that common and ludicrous Experiment, yet taken notice of by *Regius*, of immersing the small end (Fig. 13.) of a lighted Tobacco-pipe *B* under Water in a Vial, and sucking another Pipe *C*, not touching the water,

the

the Glafs being closely covered, the strong attraction of the Mouth upon the one Pipe will, through the Water upon the orifice of the immersed Pipe, draw the Smoak into the other, notwithstanding the interposition of the Water, and will put all the parts of the Water, to the very bottom of it, and the granules of Sand lying at the bottom of the Water, into motion and elevation, as in the Figure.

And the very same Answer is most clearly to be given to the third and fourth Experiments in that place, and to others of like nature, without recurring to an imaginary natural Elasticity in a small portion of included Air, whereby to expand it self without any forcible attractions to such a prodigious amplitude.

And thus I have done with what I have to say against the Solution of the *Torriceilian Experiment*, either by the Weight or Elasticity of the Air or Atmosphere; which to me seems a Supposition contradictory to our very Senses, a Supposition attended with infinite

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nite absurdities and inexplicable diffi-  
culties.

And yet, as I have before said, it is wonderful to see with what tenacity, and with how great confidence it is asserted, and how marvellously it hath obtained among the *gens literata*, and what Systems of new Physicks, what rare trains of Consequences, what Propositions, *porismata*, *Scholia*, it hath multiplied ! wherein if the Foundation it self prove but a mistake, men have spent much labour in vain upon it.

Therefore laying aside this Solution as untrue, I proceed to that which the more steady Modern Philosophers, consonant to the ancient Principles, have given in this case, and seems to be most consonant to truth.

CHAP.

## CHAP. XV.

*Concerning the true Cause of the suspension  
of the Mercury at twenty nine Inches and  
half, or thereabout, in the Torricellian  
Engin.*

I Come now to consider of that cause  
of the suspension of the Mercury at  
twenty nine Inches and half, or therea-  
bouts, in the *Torricellian Engin*.

They that assign the Diversity of the  
Weights of fluid Bodies, tell us;

1. That although Air be intrinse-  
cally heavy, yet it is neer one thousand  
times, or as *Mersennus* will have it, a-  
bout 1300 times lighter than the like  
quantity of Water; and this they en-  
deavour to evince by Experiments.  
But this Conclusion is very doubtful and  
difficult.

2. That Water is fourteen times,  
or very neer thereabout, lighter than  
Mercury, quantity for quantity, and  
this as it is easie to be tryed, so upon try-

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al I find it very neer the truth, though  
Water it self in several places differeth  
somewhat in weight, and therefore it is  
not easie to make this the common stan-  
dard for all Waters, though the dif-  
ference be not considerably great.

Though Water be ordinarily four-  
teen times lighter than Mercury, yet  
this is not the just proportion of the  
suspension of one and the other in a  
Tube.

For if a Tube of Water, above one  
and thirty foot long, stopt above and  
immerfed at the other end in restagnant  
Water, it will subside as is ordinarily  
said to thirty and one foot, others say  
more, whereof see the Experiment *apud*  
*Schottum in Technicis Curiosis, l. 3. cap. 2.*  
*Exper. 2, & 3.*

But in the case of Mercury, if the  
Tube of above twenty nine Inches and  
half long, suppose four foot, be stopt at  
the top, and immerfed at the open end  
in restagnant Mercury, it will subside  
to twenty nine Inches and half, or there-  
about.

But although in these two fluids, of  
this

this different gravity, the subsiding of either vary, yet the reason both of the subsiding and suspending of the one and the other will be the same.

Therefore what I say in relation to the suspension and subsiding of Mercury, will be in its reason applicable to the other, and therefore I shall here only treat of the suspending of Mercury, because the Instruments to try the Experiment in Water are more massy and untractable, but, as I said, the reason of one will be the reason of the other.

There be three Instances of the suspension of the Mercury in the *Torriceilian Engin*, which though they all are resolvable into one common reason, yet that reason hath a various application severally to them.

First, when the Tube exceedeth in length the ordinary station of the suspended Mercury, *viz.* twenty nine Inches and half, as suppose it be four foot long, and the Tube receive only thirty Inches of Mercury, or though the Tube be filled more than twenty nine Inches and half of Mercury, yet it is not

so

so perfectly filled, but that there remains a portion of Air in the head of the Tube, upon its inverting into the restagnant Mercury.

In this Instance, if the portion of Air be so great before its Expansion, that being expanded by the descent of the Mercury, it can reach below twenty nine Inches and half, as suppose to 20, 23, 24 Inches or less, then the Mercury will subside to that scantlet that the portion of Air is extendible unto.

If the portion of Air be less than can be extended, below twenty nine Inches and half, or the distance be so great that the extension of that portion of Air cannot by its expansion supply a greater room than what just answers to that station : as suppose the Tube were above 4 foot long, and filled with Mercury all but half an Inch, and then inverted into the vessel of restagnant Mercury, whereby before its subsiding there would be about half an Inch of free Air in the head of the Tube ; and by the descent of the Mercury to twenty nine Inches and half, if that little portion of

Air

Air were yet so much as were capable of such an extension, it would be extended to that length, *viz.* eighteen Inches and half; but if that task were too hard for so small a portion of Air to be extended unto, then the same would be supplied by the subsidiary steams and *effluvia* out of the Mercury, to make a retreat for the Mercury, to a descent of twenty nine Inches and half, as hath been shewn, and shall now farther be declared in what follows.

In this Instance these things are to be noted. 1. That by the descent of the Mercury there is no vacuity left in the place derelicted by the Mercury, as hath been shewn *Chap.* 2. That there is no new Air admitted into the Tube *ad foris*, as hath been shewn before, *Chap.* 3. But that little portion of Air is extended from the very head of the cavity of the Tube to the subsided Mercury, if it be of that substance that is capable of such an extension to the Mercurial station of twenty nine Inches and half: and if it be too little for such an extension, it is supplied by a suitable portion

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of the Mercurial steams, as hath been  
shewn. 4. But that extension or dilata-  
tion of that little portion of included  
Air, is not by any natural Elatery or ex-  
pansion of its own, as hath been shewn  
in the two former Chapters, for it is a  
portion of the free Air ; but it is a strong  
tension of that portion of included Air,  
to prevent that *casma* and separation of  
Bodies without any thing intervening,  
or the *fuga vacui*, so often mentioned by  
the Ancients. 5. And consequently,  
though in relation to the laws of the  
Universe, it be a natural expansion or  
dilatation of the Air in this circum-  
stance, *in obsequium naturae universalis*,  
yet in relation to the particular state and  
consistence of that portion of the Air  
thus expanded or tensed by the subsiding  
of the Mercury, it is forced and violent.  
As the erect posture of my Body is natu-  
ral to me, while it makes one common  
composition with my Soul and Spirits,  
and *in Concreto*, though it be violent in  
relation to the *moles corporea* of my Body,  
which would otherwise fall down to  
the Earth. 6. And the consequence of  
this

his tension of the included portion of Air (considered conjunctly with the natural inseparability of the parts of the Universe, which is all one with that which the Ancients called *fuga vacui*) is the sustentation of the Mercury, that column of descended Air, which *Linus* not absurdly calls *funiculus*, being inseparably united, and adhering to the top of the Tube at one end, and to the Mercurial Cylinder at the other end, and this *funiculus aereus* not being capable to be extended farther than to twenty nine Inches and half, being the Mercurial station, by the force of the descent of the Mercury, sustains and holds it up at that station: and so the *pondus deprimens* of the Mercurial Cylinder is equal to the *potentia superne sustinens* of the Aereal descended Cylinder upon it, in a kind of *equilibrium*, the Mercurial Cylinder by its natural motion endeavouring its descent downward, and the aereal column by a motion of restitution to its natural size and staple, retracting it as much as it may upward; and the same reason will hold of the portion of included

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ded Air, be so much as is capable of an extension below twenty nine Inches and half of the Tube: but not sufficient by such a force to be dilated to a full depletion of the Mercury out of the Tube, as possibly to 20, 15, 10, or 5 Inches from the restagnant Mercury, in which case the Mercury will subside to such a degree, and no lower.

The reason therefore, 1. Why the Mercury subsides so low as twenty nine Inches and half, its usual station, (or lower, if the portion of included Air be greater, as is before said) is, because the weight of the descending Mercurial column is of that strength, to give the included Air that tension or dilatation. 2. Why it subsides no lower, is because when the Air hath attained its utmost tension that the descent of that column of Mercury can give it (which ordinarily is at the height of twenty nine Inches and half) that aereal descended Cylinder holds it there, and keeps it up to that height, the weight of the Mercury being now lessen'd.

And that this is the cause of its sustentation.

tation appears evidently by these few Instances. 1. If the Tube of Mercury in this sustentation be suddenly lifted up from the restagnant Mercury, there will be a strong resilitio[n] of the Mercury towards the top of the Tube, that will endanger breaking it; as we see a Lute-string, if hanged upon a Nail, and overcharged with a Weight or strength at the lower end, by which it breaks in the middle, the upper part of it will forcibly resiliere in its contraction to its ordinary size. 2. If the Tube be inclined to a Level in the same parallel line to twenty nine Inches and half of the erected Tube, whereby it loseth its weight by the incumbence that it hath upon the sides of the declined Tube, the Air will contract it self to its former consistency, which it had before that tension, being by this inclination of the Tube delivered from it.

And they that object against the tension of the Air in this case; because if it had a tension in length, as the Lute-string hath, it would have likewise a contraction in its bredth, and so would

not

not fill the Tube, must remember that the Air is a tensible Body of such a nature, as is at the same time tensible in bredth as well as length, and in such its tension as closely adheres to the sides of the Tube, as it doth at one end to the top of the Tube, and at the other end to the Mercurial column.

I shall not here enlarge with Instances, to shew, that the tension of the Air, or any other tensible fluid Body necessarily effects attraction on the Bodies to which it is contiguous, but shall reserve that to the following Chapters: and I have been the longer in the explication of this Instance, because it fully opens the reason of the second Instance, which now next follows. Therefore,

2. The second Instance is, where a Tube closed at one end, above twenty nine Inches long, suppose it four foot long, is entirely filled with Mercury, and no Air admitted into it, and then inverted into a vessel of restagnant Mercury, without admission of any Air into it, either upon its inversion or immersion, (which though it be difficult,

may

may yet be done) in this case the Mercury will subside to twenty nine Inches and half in the Tube.

This derelict space of eighteen Inches and half, is, as I have before said, filled with a Mercurial steam or vapor, extracted out of the Mercury it self, and abraded from it by the descent and compression of the parts of that Mercurial column, as I have before shewn *chap.* and thereby likewise rarified or tensed, as the Air is in the former Instance.

And this being either real Air included within the pores of the Mercury, and expressed from it, or else a subtle Ingredient into the Mercurial composition, and extracted, and in its extraction dilated, as the Air is in the former Instance. It performs the same office in relation to the suspension of the Mercury, as the included extended Air did in the former Instance, and therefore it need not to be repeated.

Only I shall adde this farther, i. That it appears the extraction of the subtle *effuvia*, and their dilatation, is perform-

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ed with a great stress or straining of  
them out of the Mercurial Body, and  
with little less labour extended; which  
appears by the slow descent of the Mer-  
cury, arising from the *lucta* or contest  
that grows between the weight of the  
Mercury descending on the one hand,  
and the straining and abrading of those  
subtle particles, and their tension or  
rarification of the other hand. And the  
like slow descent there appears in  
the first Instance, especially as the Mer-  
cury more and more subsides; for then  
the Mercury having neer acquired its  
ordinary station, grows lighter, and  
the strength of its pressure begins utterly  
to languish and decay too, being neer  
equally matched with the *potentia suffi-  
nens* of the extracted subtilized particles  
in this Instance, and by the tension of  
the Air in the former instance; the rea-  
son whereof shall be farther enlarged,  
when I come to the third Instance.  
2. Whereas in the former Instance, by  
the inclining of the Tube, the Air re-  
gains its former contracted extent, here-  
by the inclining of the Tube these sub-  
tle

tle particles extracted out of the Mercury, and dilated, are not only contracted, but resumed into the Mercury from which they were extracted, as is shewn in the Chapter. 3. That upon the over sudden Elevation of the Tube out of the restagnant Mercury, there will be the like resilitio[n] of the Mercury into the top of the Tube, as in the former instance.

By this *funiculus subtilis* materi.e the Mercurial column is sustained at twenty nine Inches and half, as in the former Instance of the *funiculus aereus*.

But now how it comes to pass, that since there is yet remaining twenty nine Inches and half in the Tube, which doubtless hath a residue of subtle matter extractable out of it, to supply a greater room, that what is yet left above twenty nine Inches and half, how it comes to pass that by the weight of that twenty nine Inches and half of Mercury, there is not more of the Mercurial effluxes extracted and dilated, which may give a total descent and evacuation of the Mercurial column into the re-

Q 2      stagnat

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restagnant Mercury remains to be con-  
sidered, which shall be done in the third  
Instance which follows.

3. The third instance is, when the  
Tube is but just twenty nine Inches and  
half, or shorter, suppose 25, 20, 10, or 5  
Inches long, and filled entirely with  
Mercury, and then inverted into the  
vessel of restagnant Mercury; in that  
case the entire Mercury will remain sus-  
pended, and remain contiguous to the  
top of the Tube, and no part will de-  
scend, neither will there be any such  
*funiculus* of Air or subtle Matter to con-  
nect it to the top of the Tube.

The reason of this is what in effect  
was given in the former instances,  
though the application be different.

First, if in this instance the Mercury  
should any whit subside, either it must  
leave the *interstitium* between it self and  
the top of the Tube vacant of any Body,  
or it must yeild some subtle matter to  
supply the place it leaves abraded from  
it self, as in the second Instance. And  
if it can do neither of these, *manet sors*  
*tertia*, only to continue its station united  
to the top of the Tube. The

The former of these cannot be, for I have supposed, and shall further inforce the contradiction of the Law of the Universe to that Supposition.

Therefore it remains, that either there must be a subtle matter extracted out of the included Mercury, or of necessity the Mercury must hold its entire possession of the whole Tube.

Although the remote cause of this extraction of subtle parts out of the Mercury, in the second Instance, be that law of the Universe, to hold together the continuity of its parts, and consequently to avoid a *vacuum* or *casme* in the Universe destitute of matter; yet the immediate cause of this extraction and rarification is the descent of the Mercury from the station that it hath, and the weight of its Body is the cause of that descent.

And because it requires a strong compression of the Mercury, either to drive or draw out this subtle matter, and as it were to rake it out of the bowels of the Mercury, and to extend it, and consequently a strong weight to effect that

230 Observations touching the compression and extension , if that weight which it hath at 29 Inches and half , or under, be not effectible of such an abrasion or extension of the subtle matter so abraded ; such abrasion or extension not being effectible by that weight, the column of Mercury must hold its position, and becomes its own *funiculus*, and remain suspended to the top of the Tube.

Now it seems, that although a Mercurial Cylinder of a height above thirty Inches , as suppose three or four foot, hath such a forcible pressure , weight, and gravitation upon the restagnant Mercury, not only to drive down the Mercurial Cylinder to twenty nine Inches and half, but also to extract and strain out of it , and extend a sufficient portion of subtle matter out of the Mercurial Body to supply the room it leaves; yet that the weight only of twenty nine Inches and half, or below it, will not be able to drive, or draw out, or extend a portion sufficient for that Exigent.

And that is the reason , why at twenty nine Inches and half, or under that gage

gage, whether the Tube be just twenty nine Inches and half, or longer ; yet the Mercurial Cylinder rests there, wanting a sufficient *potentia* below that station by its weight, to extract or to rarifie any of its subtle parts , to supply the place which then it should desert or leave.

And herein the bigness or smallness of the Glass Tube, or the Mercurial Cylinder included, makes no odds ; though possibly according to the various diameters of the Tubes, one Cylinder of Mercury of 29 Inches and half high, may weigh five times another Cylinder of the same height , and yet both would remain suspended in the Tubes.

But the disparity of the *potentia deprimens*, is from the disparity of the height of the Tube. And therefore the cause why the Cylinder of Mercury, filling a Tube of 3 or 4 foot long , will subside upon inversion unto twenty nine Inches and half, and there stay, is, because before its descent to that station, its *potentia* was sufficient to abrade and extend the subtle matter, but at twenty nine Inches and half that *potentia* was spent.

And therefore the reason why the Tube of twenty nine Inches and half, or under, doth not subside at all, but remains cohering to the top of the Tube, is, because it hath not that *potentia* at that gage to abrade from it self matter to supply that place, if it should leave it.

And for this purpose it will not be amiss to take notice of what *Galileus*, and after him the Excellent *Mersennus* have given us in relation to the disparity of the *motiva potestas* of the same fluid in different positions of height or elevation.

The acceleration of motion of heavy Bodies is *ut quadrata temporum*, in which they move downwards; as if in the first Minute, a Body moves one foot, in the second minute it will move four foot, *viz.* two times two; in the third minute nine foot, *viz.* three times three; or, which comes to all one account, heavy bodies in their descent, in every given portion of time, as a minute will percur portions of space *per numeros impares ab unitate*; as if in the first minute

nute

nute it descend one foot; in the second it will descend three foot, in the third five foot, in the fourth minute seven foot, and so onward.

Mersennus partly upon this Theory, and partly upon his own experience, makes two Conclusions, (which I have experimentally found true) that are apt to explicate what I mean in this matter.

First, if there be two vessels of equal height, but of differing diameters, and with an equal *lumen* or hole in the bottom of each of them, and both filled with Water; the smaller vessel will in the same portion of time empty from it self as great a quantity of Water as the greater, for the Gravitation of the Water upon the *Lumen* is *secundum altitudinem*, not *secundum latitudinem vas*: and upon the same account it is, that if the *lumen* be at the side, and the vessels be placed in the same horizontal parallel, the Parabolical line made by salient Water out of each vessel, will be of the same length and amplitude.

2. That if there be two Tubes or vessels

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vessels of Water, *A* and *B*, of the  
height of nine foot, containing nine  
Gallons of Water, and *B* were a vessel  
of sixteen foot containing sixteen Gal-  
lons of Water, and each vessel had a  
small *lumen* in the side next to the Base,  
of equal bigness, and of that bigness,  
that were there but one Gallon in the  
Tube, it would empty that Gallon in  
one minute. In these Instances, *A* fil-  
led with Water would empty five Gal-  
lons in the first minute, three Gallons  
in the second minute, and one Gallon in  
the last minute. Again *B*, if filled with  
Water, would empty seven Gallons in  
the first minute, five in the second mi-  
nute, and three in the third minute, *viz.*  
*in ratione subduplicata altitudinis.* And  
this I have experimentally found to be  
true.

The consequence whereof is, that e-  
very fluid Body the higher it is elev-  
ated, hath the greater and more effectual  
pressure by its position upon its lower  
parts or other fluid, than what barely  
ariseth from its weight: so that in a  
lumen of Mercury in a Tube of four foot  
high,

high, though the lowest foot and the highest foot of Mercury be of an equal weight, yet the pressure of the uppermost or fourth foot, which I call its *potentia deprimens*, is to the third as seven to five, and the third to the second is as five to three, and the second to the first as three to one, namely when it hath a liberty of descending motion, as the Mercury hath in this Experiment; for the intermediate weight contributes to the pressure of the uppermost foot.

And hence it is that the power to separate and segregate, and strain, and extend the subtle matter out of the Mercury, is vigorous and effective in a Mercurial column of 4 foot high, and gradually is less and less 'till it come to twenty nine Inches and half; and then though it hath a considerable Gravitation, yet it is not equal to effect such a separation, and therefore must needs stand suspended at that height, or leave a *vacuum* behind it, which the Catholick laws of the Universe do not permit.

And thus far touching the Explication of the Solution, which seems to me to

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to be the true Solution of the *Torricelli-*  
*an Experiment.*

And in the next place I shall subjoin  
some Instances that seem to me to con-  
firm it, and answer some Objections that  
are made against it.

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## CHAP. XVI.

*Concerning the two Suppositions that are ne-  
cessary to maintain this Solution, viz.  
Natures abhorrence of vacuum, and the  
Attraction of tensible fluids, or other bo-  
dies when under a tension.*

**T**here are two Suppositions that are necessary to be proved, to render the Solution above given evident or probable. 1. The exclusion of a *vacuum* or space in the Universe wholly empty of any Body. 2. That any Body, capable of tension, or rarification by tension, (such as are the Air and thin *effluvia* of Mercury) to be attractive upon the contiguous Bodies, when actually under that tension.

i. As

1. As to the former of these, I need not much labour for it, upon two reasons. 1. Because there have been large Tractates upon this Position, and to repeat them would be but *actum agere*. 2. Because the exclusion of the Gravitation and Elasticity of the Air, invented or substituted by the modern Elaterists, doth necessarily conclude, that there can be no *vacuum* in Nature; for all those Experiments and Instances, which they produce to prove that Gravitation or Elasticity, and by which they go about to solve them, must (if those Solutions prove untrue, or ineffectual) of necessity maintain that ancient Peripatetical position, because they are incapable of any other Solution. And in my judgment that Gravitation and Elasticity of the common Air is sufficiently convinced by what hath been before said touching it.

And when I meet with those Objections that I find used by Learned men, namely how the Mercury or Water comes to know there will be a *vacuum*, if they do not leave their natural Gravity,

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vity, and so ascend or remain suspended  
to supply it, and how those stupid inani-  
mate Elements or Minerals come to un-  
derstand, that Nature so much abhor-  
reth a *vacuum*; are much of the same  
kind, as if a man should ask the Sun, if it  
move according to *Ptolemy*; or the  
Earth, if according to *Copernicus* it move;  
how they came to know that when  
they come to the Northern Tropick,  
they must move again to the Southern  
Tropick? or when it hath touched the  
Southern Tropick, it knows that it must  
come back to the Northern, and steer  
its course accordingly? Or as if a man  
should ask, how doth the Stone under-  
stand that he must descend, when yet  
all the men in the world can never  
give any satisfactory reason for its mo-  
tion to the Earth, more than to the  
Moon, but only Nature that is the *prin-*  
*cipium motus & quietis*, or rather the God  
of Nature, whose standing and statumi-  
nated Law Nature is, hath so order'd it,  
and ordered so in the best way for the  
use, beauty, and accommodation of the  
Universe. The plain truth of it is, the  
very

very consideration of the many attempts that have been made to explicate the reason of the descent of heavy Bodies, is enough to let us see, that we tire our selves in vain to determine the clear reason of it, without resolving it into the statuminated Law of Nature: though the Stone and the Earth are inanimate, or at least unreasonable Bodies, such kind of Objections therefore as these are fond and weak.

2. As to the second, namely the attraction of Bodies extended, whether by rarification or tension, especially of the Air, or this subtle matter that ariseth from the Mercury. This affection and effect is as natural as any thing in Nature, as is truly observed by the learned *Fabri*, *in primo tractatu physico*, prop. 205, 206, 208. For a strict contiguity in all parts of several Bodies is a kind of continuity of the Universe and all its parts; neither can I imagine how those that are fond of the *Epicurean hypothesis*, can better maintain the cohesion of the parts of solid bodies, their *atomis hamati* being but fancies of what men never saw in them.

The

The Air is the common cement and connecter of the different parts of this inferior world at least, and strictly interposeth between their fissures and *interstitia*, to maintain a kind of common continuity between the different parts of the Universe, and makes it as it were one continued Body.

But to come to particular Instances, which shall not be laborious but familiar and common, to evince this cohesion of the parts of tensed or rarified Air, and the contraction that it makes upon contiguous bodies, in its endeavour of restitution to its natural state and consistence.

1. The instance of Cupping-glasses, where the Air is highly rarified or evacuated by the included burning Tow, and applied to the flesh, draws up the flesh strongly unto it, when by the extinction of the Tow the rarified Air contracts it self to a narrow room, which can be no otherwise than by the cohesion of the particles of Air to the skin, and driving it in to the Glass, while it contracts it self to a narrower

com-

compass. And certainly they that tell us, that this is done by the pressure of the ambient Air upon the contiguous flesh, which thrusts it up into the Glass, either do not believe themselves while they say it, or are so transported with a love unto or engagement for their beloved *hypothesis*, of the prodigious gravitation of the free Air, that they are content to say any thing in its behalf.

*Qui amant ipsi sibi somnia fingunt.*

Shottus (who while he wrote his *mirabilia hydraulica*, was well satisfied with the Solution of the *Torricellian Experiment* by the common Solution of *fuga vacui*) was indeed afterward, when he wrote his *mirabilia Magdeburgica*, converted to the new Solution, being not willing it seems to be thought to see less in Nature than the new Tribe of *Virtuosi* that were concerned in that Engine. But for all that, this Instance of Cupping-glasses was too hard for that new Solution, and were fairly fain to have recourse to the *decanata fuga vacui*, or else it must stand unsolved, and so must any else that means an impartial and

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unpreiengaged search into Nature.

2. If an empty Bladder be tied close about the neck, and fastned to the top of the Tube, Hermetically sealed in the *Torricellian* Experiment, upon the sub-siding of the Mercury the Bladder will be expanded, not by the natural Elate-  
ry of the little particle of included Air, but by the strong attraction of the tensed Air in the Tube, by the descent of the Mercury, whereof before.

3. The Instance given in the tenth Chapter, of the Tube heated, and appended to a Ballance, so as it touch the Water, the Water rising in the Tube, the Tube will be drawn down, which is only done by the adhesion of the two extremes of the inclosed rarified Air, the one Extreme laying hold upon the top of the cavity of the Tube, the other end laying hold of the Water, and drawing it up, and therewith necessarily drawing the Tube down, whilst the included Air is contracting it self to its natural and narrower dimensions.

And because the Experiment made by a wide-mouth'd Glass would yeild some

some discoveries of this attraction by the rarified Air, which are not so visible in so narrow a Tube, I took a Beer-glas, as in the *Figure 14*, and suspended it by a Thrid to one Scale of the Ballance, and weighing it with a weight in the opposite Scale, and found it weigh two Ounces and half; then, as we used when we were School-boyes, sticking up a Candle in a Bason, I let down the Glas over the Candle contiguous to the Superficies of the Water, the Candle going out there was a portion of Water drawn up into the Glas, the Air upon the Candles going out, and the Glas drawn down into the Water, the included Air being under a cause of rarefaction, namely by the heat of the Candle, and by the extinction of the Flame now gradually contracting it self, and thereby drawing up the Water to supply that place.

The water drawn up into the glas was about half an Inch; we marked exactly the weight that was required now to lift up this Glas and included water was an addition of 4 Ounces and  $\frac{1}{4}$  of an

Ounce ; so that the whole weight that raised the Glass and Water inclosed, and severed it from the Water beneath, was six Ounces  $\frac{1}{2} \frac{1}{4} \frac{1}{8}$  of an Ounce, and then, though not without reluctance, the Glass was raised out of the Water.

Then taking the exact quantity of other Water, equall to that formerly included in the Glass, (which was not difficult to do by the help of the remaining Mark) we weighed it, and found it to weigh exactly three Ounces and half, and a very few Grains.

So that the weight of the Glass and inclosed Water amounting to six Ounces, there was requisite an addition of about half an Ounce to sever the Glass from the Superficies of the restagnant Water, which did apparently to the eye and touch adhere together, notwithstanding the counterpoise of six Ounces, equal to the weight of the Glass and included Water.

And this half Ounce of additional weight was due purely to the adhesion of the glass, Air, & inclosed water to the Superficies of the restagnant Water.

I. It

1. It seems evident, that not only the Glass, but the Water included in it, gravitates upon the Scale, for the counterpoise of six Ounces in the opposite Scale, equals the weight both of the Glass and Water inclosed in it, which could not be, unless both contributed to the *equipondium*.

2. That it is impossible that the included Water could gravitate upon the Scale, unless the rarified Air were contiguous and cohering to the top of the Glass, and also to all the parts of it included within the cavity of the Glass above the Superficies and whole body of the included Water, and also firmly adhered to the Superficies of the included Water, for they all make up but one common weight, and there must be a *vinculum* that must connect the Glass and the inclosed Water, and otherwise they could not gravitate together; so that the *vinculum* that binds the Water to the Glass is this *funiculus* (as *Linus* well calls it) of the rarified Air, as the Thrid is the *vinculum* that binds the Glass to the Scale: for otherwise the

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Glaſs would be only raised, and leave  
the Water to ſubſide into the vessel of  
reſtaignant Water, and would never hold  
an *equipondium* to more than two Ounces  
and a half, which was the juſt  
weight ſingly of the Glaſs.

But as for the fancy above mentioned,  
*Chap. 10.* that it is not the Glaſs and  
Water that make up the weight of ſix  
Ounces, but the Glaſs and the column  
of Air incumbent upon it, the vanity of  
that imagination is ſufficiently convin-  
ced in that Chapter.

3. That conſequently, the included  
rarified Air thus fastning upon the ca-  
vity of the Glaſs and the included Wa-  
ter by a ſtrict cohesion, according to the  
nature of tensed fluids, contracting it  
itſelf as much as it can by its motion of  
reſtitution, draws up the Water in the  
vessel into the Glaſs, and raiſeth it as  
high, 'till there be an *equilibrium* be-  
tween the *pondus naturale deprimens* of  
the Water, and the *potentia ſuſtinens* of  
the Air, thus now contracting it itſelf,  
and indeavouring its reſtitution to its  
juſt natural diſtination.

4. As

4. As a specimen of this cohesion of the Air and Water included in the Glass, there is also a cohesion of the Limb of the Glass and the included Water unto the Superficies of the restagnant Water, which requires half an Ounce of weight to break that continuity between the upper and lower Water. And no other imaginable cause can be assigned for it, but that *Natura agrè patitur disjunctionem partium continui.*

And as with the hand you lift up the Glass towards the Superficies of the restagnant Water, the Water included will rise with it much above the Superficies of the external Water, which cannot be by the imaginary pressure of the external Air, but by the adhesion of the column of Water to the included tensed Air, and of that to the concave Superficies of the Glass *per modum unius continui*, which any man's tryal will make more plain and evident to him, than words in writing can easily express.

But some exception is taken to the word *funiculus*, and we are asked what Grappers there are belonging to this *f-*

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niculus, that can so steadily lay hold of  
the Water and the Glass, and hold them  
thus tight together?

And I must confess, that I wonder, that any that approves the Atomical or Epicurean Philosophy, can make that Question, since all their pretence of the cohesion of the imaginary Atomical Particles in any the most solid *continuum*, are certain *atomi hamati*, that interlock one in another, which yet they never saw.

But let it be: I answer, the power of the laws of Nature are so efficacious, that they can and do colligate strictly parts even of most distantial textures and consistencies, without the help of Vellicle, Hooks, or Grappers. What are the Hooks or Grappers, whereby the Humane Soul and Body, yea the Souls and Bodies of Animals are knit together, whereby the Bloud and the Spirits are colligated, whereby the Loadstone attracts the Iron, notwithstanding an interposed Plate of Brass or body of impervious Glass, and a thousand more Instances of Attraction between Bodies, without

without the intervention of sensible Grappers and Hooks, or such gross Mechanismes.

This, and some other as light as this, and some toying with the word *funiculus*, are the greatest Objections that I find against what *Linus* hath in this matter delivered, which seem to me of no moment. Words were made to render Conceptions of things, and if they do that, they do their office.

And thus far concerning the Solution of the *Torriceilian Engin*. I shall adde a few words, as I promised, touching the cohesion of two polished Marbles, and the insufficiency of the Solution thereof given by the Elaterists, and the truth of the ancient Solution thereof.

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## CHAP. XVII.

*Concerning the Cause of the Cohesion of two  
polished Marbles.*

THESE are two Experiments, for the Solution whereof the Gravitation and Elatery of the Air is by the late Philosophers applied, which in this last place I shall examine according to my premised method.

The first is that of the cohesion of two polished Marbles. For instance, take two polished Marbles, of one dimension and figure, and exactly in the centre of each Marble let there be a Ring, whereby to suspend the upper, and whereby a weight may be appended to the lower. These two Marbles, especially if their smooth sides be a little oyled, and applied each to other, will adhere so strongly together, that the upper being suspended by its Ring to a post or frame, (Fig. 15.) the lower will sustain a weight of 20, 30, or 40 pounds, though the Marbles

Marbles be not above three Inches diameter.

And as it will do this in the open Air, it will also perform the same, though the Marbles be inclosed within a close large Glafs-Receiver, whereby it is sever'd from the common Air, or any im-pending column of the Atmosphere.

The reaſon that the vulgar Philoſophers were wont to give in theſe caſes are theſe :

First, as to the ſtrict coheſion of the two Marbles, they give this reaſon ; that the coheſion is *ob fugam vacui*. For if they ſhould be ſevered by the appended weight, the ſeparation of them would be in the ſame indiuiſible moment, be-cause they do *contingere in puncto*.

And in as much as all Bodies move ſucceſſively, and not in an iſtant, it would be imposſible for the circumam- bient Air in the moment of the ſeparation of theſe two Marbles, to pervade the whole *interſtitium* that would hap- pen upon the first ſeparation, and con- sequently there would be a mean time wherein the *interſtitium* would remain empty,

252 Observations touching the empty, therefore the cohesion is so obstinate to prevent that monster in Nature, namely *vacuum*.

And to evidence that this is the reason, they say that if the aperture be attempted at the sides, either by pulling them at one side, or by appending the weight more to one side than the other, they are easily separated, because that aperture makes an Angle, (Fig. 16.) and gradually admits the Air into it; for then the aperture is not all at once, but is *apertio continua* from the first opening. For although if the Base of the acute Angle, *viz.* A, were but a hairs breadth, the Angle were divisible Mathematically unto the uttermost extremity of it; yet it is not so Physically, it but gains a successive latitude, according as it gradually opens, and so is accommodated Physically to a successive intromission of the Air: but if the aperture were altogether, the separation in all parts of the Superficies must be Physically as well as Mathematically in the same instant, which were too quick to give an admission to the successive motion

tion of the aery body in the same instant.

But now we will consider what Solutions the late Philosophy gives in this instance.

Namely 1. That in the free and open Air there is an impending column of Atmosphere, reaching from the summity thereof to the upper Marble, and commensurate in its diameter and circumference to the *area* of the upper Marble that presseth it, with an excessive weight downwards, and so upon the upper Hemisphere if suspended parallel to the Horizon.

2. That again there is a returning or recoiling Cylinder of Air, reaching from the Superficies of the Earth upwards, of as great a *potentia sustinens* as the superior column of Atmosphere hath a *pondus deprimens*: and this inferior or resulting column is commensurate also in its diameter and circumference to the lower Marble, or brazen Hemisphere, and thrusts that as vigorously up to the upper Marble, as the upper column presseth downward. And so the *potentia sustinens* being equal to the

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the *pondus deprimens*, the lower Marble  
is able to sustain a weight equal to  
the weight of the upper atmospheric  
Cylinder ; the reason of which admi-  
rable power (according to their Suppo-  
sition) is opened *supra cap. 5.*

3. And in as much as there runs  
down as weighty and potent aerial Cy-  
linders or Atmospheric portions by  
the sides, as well as perpendicularly,  
therefore although the Marbles were  
not placed parallel to the Horizon, but  
vertically (as when each is drawn sev-  
eral ways) this lateral pressure of the At-  
mosphere is as effectual to hold the  
Marbles together in that close posture,  
as if they stood parallel to the Horizon.

4. And because they will have a  
Salve for all Sores, although these En-  
gins be placed in a close Glas-Receiver,  
whereby the aerial Columns, as well  
superior as inferior, are wholly inter-  
cepted by the interposition of the great  
Glas Receiver, the Elasticity of the  
portion of Air included within that  
Glas Receiver, undertakes and effectu-  
ally performs the *phenomenon*, by its vi-  
gorous

gorous Elasticity or counterpressure both upward, and downward, and laterally, upon the included Marbles or Hemispheres.

And this is the Solution that now obtains among the *Virtuosi*.

And upon these premises there are admirable trains of Philosophical Consequences deduced with great industry and subtlety, in this particular instance of the Marbles, which a man may read at large in the late book of *Hydrostatical Experiments*, *Exp. 13*, and elsewhere.

And now for ought I know the matter is brought to this issue, whether this or the former Solution be the truer, (for I know not a third) and if this prove untrue or uneffectual, the former of the Vulgar Philosophy must obtain, 'till some third be found out.

And therefore I will proceed in the examination of this new *Hypothesis*.

And although I think I have said enough against this whole Scheme of the Gravitation and Elatery of the Air, as it is propounded in the fourth, tenth, and eleventh Chapters; yet I shall here add some

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some things more specifically applicable  
to these Engins and Experiments; and  
remit the Reader to those Chapters, as  
to the general untruth of the entire *hypothesis*.

First therefore, as to the cohesion of  
the Marbles I offer these considerations:

1. It is plain, that the lower Mar-  
ble, if there were nothing else to sustain  
it but the recoiling Cylinder of Air im-  
pelling it upward, it would certainly  
fall to the Earth, notwithstanding that  
imaginary *potentia*; for if it were other-  
wise, it would be a necessary conse-  
quence, that though it were not conti-  
guous to the upper, it would be sus-  
tained by that *potentia sustinens*, and so we  
should have Milstones floting in the Air,  
according to the fancy of the last men-  
tioned Author of Hydrostatalical Ex-  
periments.

But the help for this is, that in that in-  
stance the impending superior column  
of Air being of equal strength with  
the recoiling column of Air, there is an  
*equipondium* between that *pondus* and *po-*  
*tentia*, and so the solute Marble or Mil-  
stone

stone, having the advantage of his own weight, and the weight of the superior column would conquer the inferior; but here the inferior Marble is protected from the weight of the superior column of Air, and so hath nothing but his own weight to contend with the inferior column, and its *potentia*, which latter overmatcheth it, and so keeps it suspended.

But this will not cure the Sore; for suppose it were two pieces of Wood, or that the Marbles were not excellently polished, so that they did not *contingere in puncto*, but some little fissure or *interstitia* did let in any small portion of Air, the Marble would fall off; and yet there the lower Marble is protected from the impendent column of Air by the superior piece of Wood, or unpolished Marble as well as here.

And to say these little filaments of interposed Air, which if all put together would not make a column of Air of a larger diameter than a small pin, should conquer the reflected Cylinder of Air, and its *potentia*, which these Masters

S sup-

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suppose to be equivalent in power to  
thirty, forty, nay one hundred pound  
weight, renders the whole Supposition  
it self to be but utterly improbable.  
Somewhat therefore else it is, that keeps  
the lower Marble suspended to the up-  
per, and not the imaginary *potentia* of  
the reflected column of Air commensu-  
rate to the *area* of the inferior Marble.

2. If the lower Marble were sus-  
tained upon the account of the *potentia su-  
stinenſis* of the inferior column of Air, and  
by force thereof it could sustain (for in-  
stance) forty pounds weight, suspended  
in the center of the Marble, it were not  
easily conceivable how if but an eighth  
part of that weight were hanged near  
the limb or circumference of the lower  
Marble, it would fall asunder, as it will;  
for the *potentia sustinenſis* of the subjected  
column of Air is applied equally to all  
the *area* of the subjacent Marble. And  
the separation will follow, if the Mar-  
bles being oyled, the one may be滑ed  
off from the other without any dif-  
ficulty; and then when they are so滑ed  
off, they are easily thereby severed,  
because

because no vacuity or instantaneous motion of the Air would follow thereupon: which could never be done, if they were so strongly wedged up one to another by so strong and powerful a counterpressure of the reflecting column of Air, which presseth up according to the new *hypothesis* with a *potentia* able to sustain neer a hundred pound weight.

3. *Stevinus* tells us in the observations upon the tenth Proposition of his Hydrostaticks, that if a vessel be made in the form of *Fig. 17.* with a hole in the bottom, and a piece of Wood lighter than Water, *viz. B B.* Cover that hole, and then let the vessel be filled with Water; as great a strength will be required by the string at *A* to move that rundle of Wood from the hole it stops, as if a weight of Water commensurate to the *area* of the rundle, from *B* to *A*, were impendent upon it. And this the Learned Author of Hydrostatical Paradoxes avouches to have tried himself, and yet the Water included is not an half nor a quarter of that weight of Water. And the column of Air that

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these Masters ſuppoſe to press the Water  
at *A*, can be no larger than the orifice  
of the Pipe at *A*, which is not one fifth  
part of the amplitude of the rundle *B*,  
nor of the hole which it ſtops. But if  
there were ſuch a reflected or recoiling  
column pressing upward to the hole of  
the vessel and rundle that covers it,  
which is five times larger than the ori-  
fice at *A*, to let in a column of gravita-  
ting Air; this rundle, and the Water  
incumbent upon it, ſhould be forcibly  
thrust up by the recoiling column of  
Air: and yet Experience tells us, that it  
is ſo far from being thrust up by the *po-  
tentia ſuſtinens* of the inferior aery co-  
lumn, that it is hardly to be raised with-  
out difficulty by a Thrid.

4. Again, take a vessel of Laton, of a  
cubical or cylindrical figure, about  
eight Inches diameter; make a round  
hole at the bottom of four Inches dia-  
meter. Then take a round piece of Bratſ  
or Lead, or ſmoothed Wood, of ſix  
Inches diameter, which may weigh two  
Ounces; wax it well, and exactly, and  
closely cover the hole, that it may over-  
lap,

lap, that no Air may go in or out by it, then rarifie the included Air, but not to any considerable height, for then growing cold it will draw up the cover when closed. Then close the top very close with Laton and Soder, that no Air can get in; the Air I suppose rarified may take up more room than its natural dimension by about two square Inches, which is more than the dimension of the Stopper: so that the supposed Elatery of the Air hath no resistance to its impelling upward, because it hath room for its reception. I say that this Stopper will not be impelled up by the pressure of the recoiling Cylinder of Air, though it weigh but two Ounces, and will be lifted up by a force or weight of three Ounces. And yet according to these mens Supposition, this recoiling column hath a *potentia* to impell or keep up forty pound, nay an hundred pound weight, being commensurate in power to an Atmospherical Cylinder of six Inches diameter, which according to their Supposition must weigh about 500 weight, namely a Cylinder of

Mercury of six Inches diameter, and twenty nine Inches high, *viz.* 783 square Inches of Mercury, which is near 500 weight. It is true I have not tryed this, but my observation of the third Experiment makes me little doubt of this.

## C H A P. X V I I I.

*Concerning the Magdeburgh Hemispheres, and their Cohesion, and the reason thereof.*

**T**HE Air is capable of tension by force, and of rarification by heat; both which agree in this, that they both give a large expansion of a less portion of Air into a greater space than is natural to it.

But herein they partly differ, and partly agree; that the immediate effect of a strong tension of Air by force (such as are the Air-pump, and the *Magdeburgh Engin*, and the *Cacabus Evacuatorius* described by *Shottus* in his *Technica curiosa*, lib. 1. cap. 5. & sequentibus) is attraction of the Superficies, to which the included tensed Air is contiguous.

But the immediate effect of rarification

of

of the Air, which is performed by heat, is a laxation of the filaments of the Air at first; for the Air being dilated, takes up more room, is more relaxed and expanded: but consequentially there follows a contraction as the heat decays, and the Air endeavours its restitution to its natural texture, and so endeavours its contraction, and consequently lays hold on the contingent Superficies, within which it is imprisoned, and attracts it with its own contraction. The instances of both which motions and effects we have in the *Magdeburgh Hemispheres* described by *Shortus, ubi supra*, and hereafter more fully explicated.

*Fig. 18, 19, 20.* The Engin is thus described. Let there be two Brazen concave Hemispheres *AB*, with rings in the Poles or center of the convex Superficies of each of them at *C* and *D*, and a ring of Leather dipt in Wax to be placed between the two Hemispheres, *viz. E*, to keep the limbs or joints of them close from admission of any Air between the commissures or joints of the Hemispheres; and a Stop-cock in the lower

S 4 Hemi-

264 Observations touching the Hemisphere at *FK*, both the Hemispheres conjoined with the Leather between them. These Hemispheres thus joined, the Stop-cock at *F* to be left open, whereby in the *Cacabus Evacuathius* or *Magdeburgh Engin*, the Air in the cavity of the Hemispheres may be as much exhausted as may be, and consequently that which remains being thereby put into a great tension, to be inclosed by turning the Stop-cock, that no foreign Air may be admitted. These Hemispheres thus ordered, will cohere so strongly together, that the strength of many Horses will not sever them, if the Hemispheres be large.

And the event is infallibly true, but the contest is touching the reason or Solution of the *phenomena*.

The modern Philosophers, with *Schottus* in his fourth Book of his *Technica curiosa*, cap. 7. q. 3. (being now a Convert to the Gravitation of the Air) tells us, that the reason hereof is, that the weight of a column of Air impendent upon the upper Hemisphere, and the reverse column of Air pressing upon

the

the lower Hemisphere upward, is the cause of this cohesion.

And that in case the Hemispheres be not placed erect, but laterally, with their Poles parallel to the Horizon, the lateral pressure of the two adverse columns of Air perform the busines, and keep them strictly together against a most powerful force indeavouring their separation.

But still with this caution, that the separating force work upon the centre of the Hemispheres, and so draw upon all parts alike: for if the *potentia separans* be applied unequally, *viz.* at one side of each Hemisphere, and not equally from the centre, then the separation will easily follow upon the application of a small separating strength, which is also most certainly true. Again, if but a little portion of Air be let into the Stop-cock, the separation of them quickly follows, which is also true.

But now the vulgar Speculators think, that this Solution is untrue, and that the true reason of the cohesion of these Hemispheres is, because that the included

included portion of Air is not wholly exhausted, neither is it possible by humane power wholly to evacuate it. But a great part thererof is exhausted, and that which remains inclosed is under a very great and violent tension, and hath a strong natural *conatus* of restitution to its natural staple and standard, and that the filaments of the Air lay hold upon every point of the concave Superficies, and draw them inward, as it endeavours its own contraction.

That there is not, nor can be, any *casma* or interval of empty space between the convex Superficies of the included Air, and the concave Superficies of the including Hemispheres, for that is contrary to the common law of Nature; and yet the Air by its intrinsick texture, and proper motion of restitution, to that just dimension that Nature hath assigned to it, affects its own contraction to its own state, and with it draws together, and holds together the ambient body including it.

Not much unlike the tension of Lute-strings, highly wound up by their Pins, which

which have a tension and contraction of the extremes, to which they are fastened, namely the Bridg and the Pins.

That the reason why, if any Air be let in, even at a Pin-hole through the Hemispheres, it is greedily sucked in by the tensed and stretched Air, to relieve it from the Rack upon which it is stretched, and being taken in it relieves that tension, looseneth its contraction, and takes off its forcible attraction upon the concave Superficies of the including Hemispheres, and gives the included Air a fair relaxation to its due size, staple, and standard; and so the cohesion ceaseth.

*Fig. 21.* That the reason why an aperture is easily wrought, when the divulsion is only of one side, is partly the same with the reason of the like divulsion of the two cohering Marbles. 1. Because the aperture is angular, and not all at once. 2. The aperture is *cunei formis*, which every body knows is more forcible than a parallel aperture. And 3. by this kind of angular opening the filaments of the Air are broken successively

cessively one after another ; but in a parallel aperture all the filaments must be broken at once, and so make one common resistance to the force. As a piece of Cloth may be torn with ease by one man, when thrid is broken after thrid, but cannot be torn by four men , when all the thrids are strained together, and so have an united and uniform strength and resistance.

And they say, that although this Engin is contrived with great pomp, yet in truth it is no more than that Experiment , which we practised with our Nutshels when we were children, where by clapping half a Nutshel to our lips, or the back of our hands, and sucking out the Air , they would stick so close, that we could not easily fillip them off.

These are the two reasons that contend for the Solution of this Experiment , the Gravitation of the Air or Atmosphere of the one hand , and the *motus* or *conatus restitutionis* of the Air, and the natural inseparability of Bodies without any intervening Body of the other

other hand. If the former be laid aside is untrue, the latter needs no great labour to prove it; for these be the only two Competitors for the Solution of this Instance, that I ever heard of.

And I think it is very plain, that the imaginary gravitation of the Air, neither hath nor can have any thing at all to do in this busines.

And because I had not those Evacuatory Engins before described, and if I had had them, yet I would not have singly rested upon them, without making trial by rarification by heat, I took the method hereafter following, for the full examination of this Instance.

For it is obvious beyond dispute, that that if there be a cohesion wrought after rarification of the Air by heat, in the Instances hereafter given, as there is when there is a tension of the Air by a forcible Evacuation, as in the former Instance; the true cause of each cohesion must be one and the same. And if the *pondus externi aeris* contribute nothing to the cohesion of the Hemispheres after rarification of the included Air

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Air by heat, it contributes as little to  
the cohesion of the Hemispheres forci-  
bly evacuated by an Air-pump, or the  
*Cacabus Evacuatorius* before mentioned.

And on the other side, if the cohesion  
of the Hemispheres after rarification by  
heat be caused by the *motus restitutus*  
of the included rarified Air, and by the  
strict continuity (as I may call it) of the  
filaments of the Air to the cavity of the  
Hemispheres, to avoid that Vacuity  
which would otherwise happen by the  
contraction of the Air into a less room  
than would fill the whole cavity; the  
same is unquestionably the reason of the  
cohesion of the Hemispheres forcibly  
evacuated as above.

The Instances therefore that I tryed  
are these that follow, which I shall men-  
tion severally, with my observation to  
each of them. For if I mistake not, this  
one Experiment, duly improved, wholly  
sets aside the imaginary Gravita-  
tion of the Air, so usually applied to  
the Solution of the Torricellian Ex-  
periment, the cohesion of Marbles,  
the raising of Water in Pumps, and

by Suction, and many more of like nature.

First, I caused two Brass concave Hemispheres to be cast, each of six Inches and a quarter in the cavity, and about seven Inches diameter in the convexity, exactly closing one to another on their rim, and the lower let up into the cavity of the upper about half an Inch, for their more exact closure; and the outward rims of each about a quarter of an Inch in breadth, exactly closing one upon another, so that being closed they made one perfect Sphere, according to the form of the *Magdeburgh* Hemispheres above described.

The lower Hemisphere weighed six pounds, seven Ounces, and a half, *viz.* 103 Ounces, and half; the upper weighed only six pounds.

I then took a circular piece of Calf-skin, a little above half an Inch broad, and in length sufficient to compass the Hemispheres, and dipping it in melted Bees-Wax, with a very little Rosin, so that it might just lye between the commissures of the two Hemispheres, and

stop

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**stop out all access of the forreign Air be-**  
**tween them.**

First therefore, without any thing more done, I applied the two Hemispheres each to other, with the interposed girdle of waxed wet Leather, and pressed them close and hard together, that no commerce might be between the included and the common Air.

And so there neither was nor could be any force of compression used to the included Air, because 'till the closure of the Hemispheres it had a free intercourse with the external Air, and when perfectly closed it was but just as much as the Hemispheres would freely contain, and consequently of the same texture with the forreign Air.

In this Instance, the upper Hemisphere raised gently by the ring, there seemed a little weak cohesion between them, because the rims were perfectly closed, and the separation was altogether in the same moment, but the cohesion so faint and weak, that it would not sustain the hundredth part of the weight of the lower Hemisphere, and scarce perceptible. And

And the reason of the disparity between the cohesion of these Hemispheres, and that before mentioned of the 2 polished Marbles, are these. 1. Because the contiguity of the two polished Marbles is in all points of their Superficies, but here the contiguity of the rims of the Hemispheres was but in a circle of a quarter of an Inch broad, and so the forreign Air had a shorter journey to go in the moment of the separation of the Hemispheres, than in the moment of the separation of the polished Marbles. 2. Because the contiguity of the polished Marbles is without any interposition of Leather, which possibly would impede the firmness of the cohesion of the two Marbles themselves.

But now if the pressure of the aereal Cylinders either in the polished Marbles, or in the instances of the Hemispheres, before and hereafter given, were the cause of their cohesion, what imaginable reason can be given why these two Hemispheres should not cohere as firmly in this Instance.

T

For

For though there be Air included between the two Hemispheres, yet all communication between the included and forreign Air is perfectly excluded by walls of Brass, and the impervious Leather girdle.

If it be laid, that the Elasticity of the included Air resists the compression by the forreign Air, and so impedes the cohesion.

I say if any such Elasticity were admitted in the Air, (which I do not) yet most certainly the included Air hath no greater Elasticity than the common Air, whereof it was but just now a portion, and hath received no more compression by the juncture of the Hemispheres, no more than Air in a Bottle covered with a flat Board or Paper.

The consequence whereof is, that the Elasticity of the forreign Air is as strong as the Elasticity of the included Air, and must needs keep it in *aequilibrio*. And the consequence thereof is, that if the *pondus externi aeris* (if it were any way considerable) would have its free exercise upon these Hemispheres, though filled with

with Air; for according to these Masters supposition, the Cylinder of Air of each side pressing each Hemisphere, is above seven hundred pound weight, which it may freely exercise, the imaginary Elasticity of the included and foreign Air being *ex suppositione* equal. But the ensuing Experiments will render this more evident.

Secondly therefore, I took a handful of flax, and leaving the mouth of the lower Hemisphere upward, I put in the flax, and gave fire to it, holding the upper Hemisphere close over it, whereby the Air in both was greatly rarified, and a considerable portion of the space taken up by the flame of the flax. Then clapping the upper Hemisphere upon the lower, with the interposed Leather girdle, I pressed them together with all the strength I had, neither were they possibly capable of a nearer or closer contiguity to the interposed Leather, though the imaginary Cylinder of the Atmosphere had pressed them, and hereupon the flame and fire was presently extinguished.

The Events were as followeth.

1. There grew a present cohesion of the two Hemispheres together, even while they were hot, and the included Air under a high degree of rarification by the heat, and so could not have any considerable tension while the heat continued.

And the reason of this cohesion was plainly this. That the fire and flame had not only rarified the Air included within the compass of the Hemispheres, to an expansion far greater than was natural to it; but also the flame and fire took up a considerable portion of the space between the Hemispheres, which being extinct in the moment of their juncture, that very rarified Air that was within, was forced to take up so much more space as the flame took up before its extinction. And from thence grew necessarily an attraction upon the cavity of the Hemispheres, though that very Air was under a rarification by the continuing heat.

2. But this cohesion was but weak, because the Air was then under a strong

strong dilatation by the continuing heat, and therefore the upper Hemisphere lifted up by the ring, would not sustain the weight of the lower Hemisphere.

3. But when the Hemispheres thus heated, and the included Air greatly rari- fied thereby, were permitted to continue together closed 'till they were perfectly cold, and thereby the Air had opportunity to contract it self, or at least to endeavour it as much as it could. Now the cohesion of the Hemispheres grew strong, so that the upper lifted up would raise the lower in conjunction with it, with the addition of some weight appended to the lower ring. And the reason was, because the heat which was the cause of the great dilatation of the included Air was now cea- sed, and the Air by its motion of re- stitution endeavoured to contract it self to its natural Expansion.

And because the included Air (it may be) was expanded to above four times its true natural size and dimension, and now endeavoured to contract it self into

into that space, but could not attain it, unless three fourths of the space included within the Hemispheres should have been left perfectly empty, the Air endeavouring its contraction, according to the particular inclination of its own motion; yet by the common law of the Universe was kept under its former Expansion, to prevent the *casma* between the Air and the concave Superficies of the Hemispheres, and so as much as was possible attracted upon them, and kept them together, no foreign Air being admissible for the relief of the included Air, to help to restore it to its due size and texture: as a living Oyster contracts its shell together by its own contraction within its shell, when a person is opening it. The comparison is homely, and suits not in all things, because the Oyster is a vital Being, but yet it serves to explicate the method of this attraction, by the filaments of the Air upon the contiguous concave Superficies.

And from this instance of fired flax (resembling very much the common instance

instance of Cupping-glasses,) it appears plainly, that in those Glasses the flesh is not driven up by pressure of the forreign Air upon the glass or the contiguous parts of the flesh; but purely by the attraction of the included residue of Air upon the extinction of the flame. For the Hemispheres were at first pressed together as close as they could be, and no man can imagine that the external Air can have any influence through these Brazen Walls, that are neer half an Inch thick, where yet the Attraction upon the included Superficies is visible and apparent; and in this and my other Experiments it was not possible for the Hemispheres to receive a greater or cloſer contiguity than I gave them on their first union: and therefore I do not wonder, that *Schottus*, though too hastyly subscribing to the pressure of the Air, as a Solution of some Experiments, yet in his *Technica Curiosa*, l. 4. cap. 12. ¶ 1. is forced to confess *carnem & sanguinem subintrare cucurbitas, nec pressione Aeris externi carni circum illas incumbenti, nec pressione aeris illas incumbentis & deprimentis,*

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primentis, ac intra carnem mergentis, sed  
suctione & attractione.

3. Because I would improve the Experiment to the uttermost, I made a trial with a stronger heat. I therefore put burning Coals and red hot Iron into each Hemisphere, 'till they were hot as I could well touch them, and then casting out the Coals, the Air within the cavity of each Hemisphere was highly rarified; then clapping the Leather girdle about the rim I closed the Hemispheres very close, as my strength could force them together; the Events whereof were these:

1. While the Hemispheres were hot, and so the included Air under a high degree of rarification, there was very little or no cohesion, for the Air was under the same laxation as at first, and therefore they would presently fall asunder, the upper being raised by the ring.

2. As the heat decayed, and the Hemispheres grew gradually colder and colder, and consequently the Air more and more endeavours to contract it self, and

is a consequence thereof attracted more forcibly upon the cavity of the Hemispheres, the stronger was there cohesion; because as the Air grew colder, it endeavoured more and more to acquire its just natural dimension and texture.

3. At last, when the heat was quite extinct, and the Hemispheres grown perfectly cold, the cohesion was so strong, that the upper Hemisphere suspended upon a hook, did not only sustain the lower Hemisphere, which weighed neer seven pounds, but also a weight of twenty eight pounds more, in all about thirty five pound weight, before they would be disjoined.

For now the Air being discharged of that heat that kept it lame, and without a bent, or tension, or contraction inward, did now endeavour its contraction strongly, and therewith pulled inward the Hemispheres, from which it could not be severed, by reason of common Natures declination of a Vacuity.

4. My fourth Tryal was this, that when

when the Hemispheres were first heated, and clapt together, and thereby when they became cold, cohered exceeding strongly; yet when they were under that strong cohesion, that they would sustain a considerable weight as before, without parting asunder, yet if afterwards they were brought but to a moderate heat, either by a chafing-dish of Coals put under them, or by holding them neer the fire, they would suddenly fall asunder of themselves, without any weight appended to sever them.

The reason whereof is apparent, namely that by the heating of the Hemispheres the included Air is again expanded, and its tension lax'd, and the filaments thereof which were before under a tension and contraction by the Air, striving to gain its natural temperament, are now let loose, as a Lute-string is let down by the unscrewing of the Pins. And consequently the suction and attraction of the Air upon the concave Superficies of the Hemispheres, which before held them together, is relaxed and discharged.

And

And thus we have the true *phenome-*  
" *nus* of this Engin, and, as I take it the  
true reason and solution of them.

And upon these Instances I do con-  
clude,

1. That men have had very little  
reason to conclude the Gravitation or  
Pressure of the Air upon the inferior  
World, or the parts thereof; for the co-  
hesion of these Engins is most plainly  
upon another reason, as appears by what  
is before declared.

2. That this instance is so far from  
concluding the Gravitation of the Air  
in that measure, that the late Masters  
contend, that plainly that Supposition  
neither hath nor can have to do in the  
solution of these Instances.

For most unquestionably, the Air  
doth as much gravitate (if at all) upon  
the Hemispheres, when the included  
Air is rarified by heat, in these Instan-  
ces, as it doth when the Hemispheres  
are grown cold; and yet in the former  
case there is no cohesion, but contrari-  
wise a separation of the cohering He-  
mispheres if again they are heated.

3. That

3. That the cause of the cohesion therefore is not *ab extra*, or *de foris*, but *ab intra*; namely, one specifical to the Air, whereby becoming cold after a great expansion by heat, it endeavours its contraction to its due space and texture, by a motion of restitution; the other common to all parts of the Universe, that common law of Nature that preserves contiguity, or indeed rather continuity of the parts of the Universe: and therefore suffers not the Air to contract it self, so as to leave an empty space between it self and the concave of the Hemispheres, whereby it comes to pass, that the Air endeavouring to contract it self, layes hold upon, and endeavours to contract and hold together all parts whereunto it is contiguous, when no access of forreign Air is admitted.

4. That the least admision of forreign Air, though through a Pin-hole, in the Hemispheres, gradually gives a relaxation of the tension of the included Air, which greedily sucks it in, and thereby is delivered gradually from its tension, and lets go its hold of the contiguous

gious Superficies, which is the natural and necessary effect of that tension.

I cannot easily perceive how these Conclusions against the imaginary Gravitation of the Air can be evaded.

If it should be said, that the reason why the Hemispheres cohere not when they are hot, is, because the convexity likewise of the Hemispheres are also hot, and so retard the Gravitation or Pressure of the Air upon them by their heat, which is not so done when they are cold.

I answer 1. That though the convex Superficies were admitted to be as hot as could be indured, yet the Sphere of that Activity is not two foot in circumference, which could never break the pressure of a column of Air of at least seven Miles high, and near a thousand pound weight, according to the Supposition. 2. Neither is the heat required to make such a disjunction of that Extremity, as to make any such impression upon the Air; for if the Hemispheres be made but bloud warm after their cohesion, they will fall asunder.

3. But

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3. But wholly to prevent this imagination, if the Hemispheres be cased over with a flat or spherical Cover of Wood, Lead, or Brass, which may receive the supposed Gravitation of the Air, yet the cohesion, while the air is hot within, will not last; for it is the laxation or relaxation of the tension of the Air within that, gives the separation, or easie separability of the Hemispheres.

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CHAP. XIX.

*Concerning the raising of the water by Pumps or Syringes, and the Cause thereof.*

**I**N the two former Chapters I have considered the cohesion of polished Marbles, and the *Magdeburgh* Hemispheres, both in truth resting upon Solutions somewhat of the same nature. And therein I have as I think excluded the Gravitation of the Air, or Atmosphere, or aereal Cylinders, from any contribution thereunto.

I have chosen in this Chapter to consider

ider the Elevation of Water in Pumps and Syringes, both which are in truth but the same Engin, though circumstantially varied.

And although I shall herein have occasion to say something touching the Gravitation of the Air, and shall in some measure evidence, that the Water is not raised in these Engins by the weight of the Air, pressing upon the restagnant Well or other vessel of Water, yet I shall not labour that, much, in this disquisition, because enough hath been said thereof before.

But my principal scope herein is to discover, even to sense, that there is no such Elasticity in a separate portion of common Air as the Elaterists suppose, whereby to salve the suspension of a column of twenty nine Inches of Mercury in the *Torricellian* Experiment, when the same is pent up in a close vessel or room, every where excluding the imaginary pressure of the Atmospheric Cylinder; and consequently that the Solution of that Experiment, either by the Weight or by the Spring of the Air,

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Air, is a mistaken Solution, and hath  
no reality in it.

The raising of Water by common Pumps or Syringes (especially as it is this day ordered, sometimes with double, sometimes with single Valves, to hinder the regress of the Water) is so well known even to ordinary Plumbers, that it needs no accurate description.

But the reason of this Elevation of the Water is variously rendered, according to the different *hypotheses* that men have entertained.

The ancient and common, and I think the true reason thereof, is this:

The lower end of the cavity of the Pump or Syringe being immersed below the Superficies of the stagnat Water, there is a Cylinder of Air in the cavity of the Pump or Syringe, between the Superficies of the Water and the bottom of the Bucket and upper Valve of the Pump, or between the Superficies of the Water, and the bottom of the *Embolus* of the Syringe closely penned up.

That by the Elevation of the Bucket

in the Pump, or *Embolus* in the Syringe, that portion of intermediate Air is under a tension, and draws vigorously upon the Superficies of the restagnant Water, and that again upon the continuous Water, and so the tensed *funiculus* of the Air raiseth a *funiculus* of Water, commensurate to the cavity of the Pump or Cylinder, which again by depression of the Bucket riseth through the Leather-valve that covers it, and by an iterated elevation of the Bucket or *Embolus* draws up more, whereby there is a continued supply of a *funiculus aquae*, which is finally thrown out at the orifice, or nose of the Pump.

That it is a certain truth, that Water is raised either by a *pondus* or pressure, that is equivalent to the weight of the Water that is elevated, as in the Engins where Forcers or pressures are used to raise Water; or else by an attractive *potentia* equal to the weight of the Water drawn up, as in Pumps and Syringes.

That consequently, the *vis* or *potentia suctoria* or *attractiva*, will raise such a weight

290 Observations touching the weight of Water as is less than this *vis* or *potentia*, (for whether the Elevation of Water be by a *vis deprimens* as in pressing Engins, or *vis suctoria* or *attractiva* as in Pumps or Syringes, or the preponderation of the *potentia* to the weight of the Water) thus it sustains, impels, or attracts, and therefore they are all reducible to the reason of the *libra* or Ballance.

And on the other side, if the weight of the Water exceed the *pondus* or *potentia* which must raise it, it cannot either draw, or impell, or keep it up beyond an *equilibrium* with that *pondus* or *potentia* *impellens* or *attrahens*.

And therefore if there be an excessive distance between the Superficies of the stagnuant Water, and the bottom of the *Embolus* or Bucket, suppose it 50 or 60 foot of perpendicular height, whereby the *potentia attractiva* is overmatched with a *pondus* of Water more than its strength, it will not raise it to a due height. For still as I said, where any weighty body is raised out of its place, it must be by a greater weight, or

at

at least by a *potentia*, exceeding that weight of the Body to be raised.

But the modern Philosophy tells us, that the raising of the Water in the Pump or Syringe, is not by a *vis attractiva* or *suctoria* of the Bucket or *Embolus*, but that it is forced up by the weight of a Cylinder of Air, commensurate to the cavity of the Pump or Syringe.

And that the Elevation of the Pump or *Embolus* of the Syringe, attenuating the Air inclosed in the cavity of the Pump or Syringe, and thereby disabled by its laxity to resist the pressure of the external Cylinder of atmospherical Air, the Water is driven up into the cavity of the Pump or Syringe.

And because a Cylinder of Mercury of twenty nine Inches and half, is by them supposed to be of equal weight to the Cylinder of Air of like diameter, reaching from the top of the Atmosphere unto the vessel of stagnating Mercury in the *Torricellian Experiment*, and a Cylinder of Mercury of twenty nine Inches and half high, countervails in weight a Cylinder of Water of like

diameter of about thirty three foot high, and consequently a Cylinder of the Atmosphere counterpoileth a Cylinder of Water of about thirty three foot high, therefore the Water in a Pump will be raised but just to that height, *viz.* neer thirty three foot, and no higher, because such a column or Cylinder of Water is counterpoised by an Atmospherical Cylinder of like diameter. Thus I think it is rendred by *Schottus* in his *Technica Curiosa*, l.4. cap.8. q.4. and by the Excellent Author of *Hydrostatical Paradoxes*, n. 5. and by other learned men of this latter age.

But under their favour I think this is not the true Solution of the instance in hand, but the first is the true Solution thereof.

If the elevation of the Water in the Pump may be more considerably higher than 33 foot, certainly this latter Solution cannot serve. And though this possibly hath not been fully tryed, and requires exact Engins to effect the Tryal to the uttermost; yet there are many that do experimentally affirm, that

that a Pump or Syringe well ordered will raise Water above forty foot perpendicular height ; and if so, the counterpoise of the aereal Cylinder will not do the busines. But this I have not tryed, I will therefore apply my self to what I have tryed.

*Fig. 22.* I took therefore a great and strong globular glass Bottle, *ABC*, holding five Quarts of Water, of about nine Inches Diameter, with a double orifice, *viz.* one at *A*, wherein to fasten my Pump hereafter described, another at *B*, to let in or pour out Water, to leave open or close, as I saw cause.

In *A* the neck of the Bottle I fastned a Laton Pump or Syringe, of about an Inch and a half diameter, marked *D*, with a small Pipe about half an Inch diameter, reaching neer the bottom of the Glas, *viz.* *E*; an *Embolus* at *G* reaching down neer *H*, and a little crooked Pipe at *H* to drive out the Water by the depression of the *Embolus*.

This Pump was fastned and strongly luted in the neck of the Glas with a double Valve, one about *A* to receive the

Water into the Pump, and to sustain it from returning, by depressing the *Embolus*, another at *K* the top of the orifice, to keep the Air from entring upon raising the *Embolus*.

So that now the Bottle was the Well, which I filled up with Water.

Therefore leaving open the orifice at *B*, whereby the Air had free access into the Bottle, by lifting up and depressing the *Embolus G* the Water was freely and easily without any difficulty drawn out at the orifice *K*, through the Valve there placed, and so I emptied half the Bottle of Water, namely to *I* the superior half, being full of free uncompresed and common Air; and the lower half at *I* being full of Water.

The Pump being in this posture, I took a short piece of Cork, first boyled and then dipt in melted Bees-wax and Rosin with it, I closely luted up the orifice at *B*, so that no air could possibly enter. And then I lifted up and depressed the *Embolus* several times, so long 'till I could see the whole success of this Tryal.

First, the *Embolus* was raised, but not with

with equal facility, as it was done when the orifice *B* was open.

The reason whereof is, because every elevation of the *Embolus* gave a violent tension to the Air in the superior cavity of the Glafs, which upon every portion of Water drawn up must necessarily be expanded into a larger space to supply the space left by the raised Water. So that the expansion of the Air was not natural, or of its own accord, but by a violent attraction wrought upon it, and tension of it.

Secondly, yet notwithstanding the exclusion of the Air, I did freely pump out above a quart of Water, whereby the Water subsided an Inch and a half below *I*, and consequently the Air expanded and dilated to so much more space than it had before, *viz.* whereas the superior cavity of the Glafs held by the Air was two quarts and half, now it took up the room of three quarts and half, and somewhat more.

So that here was a quart of Water raised up by the suction of the *Embolus*, and yet no possible Gravitation of that

imaginary atmospherical column to press or drive it up ; so that surely the ascent of the Water in the Pump is not by the Ballance, *equipondium*, or preponderation of a column of the Atmosphere to the column of Water raised in the cavity of the Pump, for here was no access for any such column : whether the imaginary Elasticity of the included portion of Air may be called in to help the matter, we shall see hereafter.

Thirdly, but when I had raised up this quart of Water, and emptied so much out of my Bottle, all my iterated tractions and trusions of my *Embolus* could raise not one drop more.

But all that was done by these repeated motions, was only the rarifying of the Air included in the Pump, when the *Embolus* was lifted up, and condensing it again as the *Embolus* was depressed, but no Water raised.

Now in this instance, the reason why a quart of water was raised, though the orifice at *B* were stopped, was because the *potentia suctoria* or *attractiva* of

of the Pump was strong enough to give a dilatation of the Air to a greater space by a quart than it had before.

And the reason why it could raise no more, is this, namely, in as much as every portion of Water raised out of the vessel, must of necessity leave a space devoid of so much Water as is so raised up, and that derelicted space must either be wholly vacant of any Body, which by the universal law of the Universe is prohibited, or else the Air must expand it self to fill it. And in as much as the Air having undergone as great an Expansion already (by the evacuation of the first quart of Water) as this *potentia* or *vis attractiva* was able to effect. The included Air thus expanded resisteth any further expansion with an equal strength to the attractive power of the Pump, and so renders it *conamen irritum*, to give it a larger expansion, and consequently no more Water is raised.

But if a greater strength were applied, strong enough to master that resistence, as if the Pump were longer, that it might

might give a stronger suction, that might overmatch the resistance of the included Air against a higher degree of tension; yea or if the superior Air, after this quart of Water was evacuated, had a strong external heat applied to it, whereby it might be capable of a larger expansion, the Water (while the Air was under this forcible expansion) would be as easily raised by the Pump as at first. But this I durst not try, for fear of breaking my Glass, or unluting of it.

Upon this consideration and tryal it is manifest,

First, that the Gravitation or pressure of the external Air is not the cause of raising the Water in the Pump; for here it is raised where there is no Gravitation, the orifice at *B* being perfectly stopt.

Secondly, that the common Air hath no such Elasticity, as the modern Philosophy assigns to it; for here a portion of Air included in the Glass is of the same texture with the common Air, and a portion of it, and yet evidently there is

is no such Elasticity in it as is supposed.

And this appears, 1. because it is not expanded to one third part of greater space than it had before, without a force offered to it, as above is shewn. 2. When it is expanded to a third part of space more than it had, it is so far from a natural expansion of it self farther, that it resists the *attractiva potentia* of the Pump to give any larger expansion. And 3. if it had yet a natural expansive power without force or violence added to it, it would certainly drive up the rest of the Water that remains, especially when the Air in the Pump is rarified or extended by the elevation of the *Embolus*, and yet no Water follows it, in this instance, which yet it would do, if the Air had a continuing Spring or Elasticity to drive it up.

It is true, that naturally all the spaces of this lower World (at least) are and must be filled with some body or other: and as one body moves or is moved out of one space to another, some other body succeeds in that space that is so left, and fills it. And the fluid Bodies of Air and Water,

Water, or some fluid bodily substance, equivalent to either of them, are in respect of their fluidity fitted more than other solider Bodies, to supply and make good the *interstitia* of other Bodies; and if they be contiguous unto them, to slip into the spaces deserted by their motion into other spaces, and accordingly will do it, if they are not impeded by the resistance or interposition of other more stubborn and untractable Bodies. And by this means upon the successive motions of Bodies, there is maintained a successive and continual circulation in the motions, especially of those Bodies that are fluid, and so more ready and apt for motion.

In the Instance of my Pump, suppose that the orifice at *B* be left open, and the vessel or Bottle have a Gallon of Water in it, and there stands by it an empty vessel, suppose *M*, that contains a Gallon of Air: if I pump out a Gallon of Water out of the Pump into the vessel *M*, as the Pump is gradually emptied of the Gallon of Water into *M*, the Water leaves its place in the Pump, and takes

up as much room in *M*, and the Air in *M* leaves as much space there, and takes it up in the Pump, to which it hath a free access; so that the two Elements change their stations, and while they hold their natural texture, hold the same extension of space, though not the same situation. And thus a circulation is naturally consequential upon the motion especially of fluid Bodies; and therefore when the Water leaves its place in the Pump, and dispossesseth the Air in the vessel *M*, and the dispossessed Air gradually succeeds into the space left by the Water, as fast as the Water leaves it, and takes up another room equal to what it had before, though in another *ubi*; here is no Gravitation of an Atmospheric column upon the orifice *B*, but only a natural motion and succession of the like portion of Air into the place reclined by the Water, and a natural desertion of that space which it before had in the vessel *M*, which it cannot retain against the weightier and more prevalent Element of Water taking it up. So that it is neither a Gravitation

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tation of the atmospherical Cylinder  
upon the orifice *B*, nor any great Ela-  
sticity of the Air that drives up the Wa-  
ter, but a natural circulation of the por-  
tion of Air into the space derelict by  
the Water ; and the immission or in-  
nuation of the Air into that Cavity, is  
the Effect not the Cause of the recession  
of the Water.

And now to reduce all this long pro-  
cess to the thing I principally intended,  
we are told by the late Philosophy, that  
the suspension of the Mercury at twen-  
ty nine Inches height in the *Torriceilian*  
Experiment, is caused by the counter-  
poise of the weight of an external atmo-  
spherical Column, commensurate in its  
craffitude and weight to the Mercurial  
Cylinder suspended in the Glass Tube.

And when it is replied, that the like  
suspension happens, if the Mercurial  
Engin be placed in a close room, yea or  
in a Glas every way closed, that admits  
not any such Mercurial column, but in-  
tercepts it and its pressure by its closure  
every way. We are again told, that  
that little portion of Air included with-

in that room or Glas, hath so potent an Elasticity, that it will as effectually suspend the Mercury in the Tube, as that tall Atmospherical Cylinder of at least seven Miles long. This I say seems to me utterly incredible for these reasons:

1. The Air included in a close room or Glas, hath only the advantage of its imagined Elasticity, but not the advantage of the stupendous weight assigned by this New Philosophy to the external aerial Cylinder.

The external Air hath the same Elasticity attributed to it, as the Air included in the close room or Glas, and also the supposed advantage of the weight of the Cylinder of the Atmosphere besides.

If the bare Elasticity of inclosed Air be sufficient to suspend the Mercury at twenty nine Inches and half, within the room or Glas; the same Elasticity in the external Air, with the advantage of its weight, should suspend the Mercury to fifty nine Inches high, which was never yet done.

2. If the Elatery of the Air cannot drive

304 **Observations touching the**  
drive up a successive portion of Water  
of half an Inch diameter into my Pump,  
without the attraction of the *Embolus*;  
nor yet is that attraction also effectual to  
raise the Water, when it hath expanded  
the Air to about a fourth part of space  
more than is natural to it. It is impossible  
that the Elasticity of the Air, includ-  
ed in a close room or Glass Receiver,  
can drive and keep up a Cylinder of  
Mercury twenty nine Inches high,  
which yet according to the amplitude  
of its diameter may weigh, two, four, or  
ten pound.

I do therefore conclude upon the  
whole matter, that it seems to me, the  
new *hypothesis* of the Weight and Elas-  
ticity of the Air, as it is delivered by the  
late Philosophy, is an Ingenuous Inven-  
tion, but wants Reality to support it  
self, or to give a right and true Solution  
to the *Phanomena* wherereunto it is  
applyed.

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Books printed for, and to be sold by  
*william Shrowsbury*, at the sign of the  
*Bible in Duck-lane.*

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**A**N Essay touching the *Gravitation* or *Na-*  
*Gravitation* of Fluid Bodies, and the rea-  
sons thereof, in 8°.

*Stereometrie*, or the Art of *Practical Gauging*,  
shewing in two Parts, 1. Diverse facile and  
compendious ways for Gauging of *Tuns* and  
*Brewers Vessels*, of all Forms and Figures, ei-  
ther in whole, or gradually from Inch to Inch,  
whether the Tun or vessels Bases above and  
below be *Homogeneal*, or *Heterogeneal*, par-  
allel and alike situate or not. 2. The Gauging  
of any *Wine*, *Brandy*, or *Oyl-Cask*; be the  
same assum'd as *Spheroidal*, *Parabolical*, *Ca-*  
*nical*, or *Cylindrical*; either full, or partly  
empty, and at any Position of the *Cask*, or Al-  
titude of contained *Liquor*; performed either  
by brief *Calculation*, or *Instrumental Opera-*  
*tion*.

Together with a Large *Table* of Area's of 1  
Circles Segments, and other necessary *Tables*,  
and their excellent Utilities and Improve-  
ments;

[ ]

ments ; with a Copious and Methodical Index of the Whole ; rendering the Work perspicuous and intelligible to Mean Capacities. By John Smith, Philo-Accomptant : in 8°.

Enonis Burgersdicii *Idea Philosophiae Naturalis, sive Methodus definitionum & controversiarum Physicarum. Editio Novissima.*

*Ex accessit Idea Oeconomica & Politica Doctrina, eodem Auctore. Opus Posthumum. 12°.*

*Antiqua Ecclesie Britannicae Libertate, atque de legitima ejusdem Ecclesie exemptione à Romano Patriarchatu Diatribe per aliquot Theses deducta : Autore I. B. S. S. Theologiae Professore. 4°.*

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Some Occasional  
ADDITIONS  
TO  
DIFFICILES NUGÆ:  
OR  
OBSERVATIONS  
ON THE  
TORRICELLIAN Experiment.

CHAP. XX.

## Concerning Siphons, and the Motions of Liquids in them, and the reason thereof.

Having in the three last Chapters of this Book given some account of the three Experiments, of the Co-besion of Marble, the *Magdeburgh* Hemispheres, and the closed Pump, in order to the discussion of the Question,

2      **Observations touching the**  
touching the *Gravitation and elasticity*  
*of the Air.* I have thought good to sub-  
join this twentieth Chapter, touching  
the old Engin called the *Siphon*, and the  
motion of Water in it; partly, because  
it is a busines of the same kind with  
that which hath been formerly handled,  
and partly, because learned persons of  
differing Judgements and Opinions,  
have appropriated and drawn over this  
*Phænomenon* among others, in favour  
of their different suppositions.

This subject was first, that I know  
of, handled by *Hero Alexandrinus*, in  
his *Spiritalia*, and among other *Hy-  
draulic* Engins: But of later times,  
by many excellent persons, as *Mersen-  
nus*, *Sir Kenelm Digby*, *Regius*, and the  
late great improver of Experimental  
Philosophy, *Mr. Boyle*.

I shall select some few Observations  
touching it, and proceed to the ex-  
amination of the reason of them.

*Siphons* are of two natures or kinds,  
one inverted, whose Leggs are turned  
upward; and the other I call erect,  
whose Leggs are turned downward;  
which

which latter, is the principal subject of this Chapter, wherein I will be as plain as the matter will, though, if men find fault with Expressions, without regarding the scope, it will obscure the truth.

The Inverted *Siphon* bears some resemblance to this inserted, made of Glass, Copper, Lead, &c. wherein the rule is generally true, that



Water poured into it, alwayes obtains the same level, or a Superficies equally distant from the Centre of the Earth in both Leggs.

And though one of the Leggs be of greater Amplitude than the other, and contains five times the portion of Water, that the other contains, yet it doth not gravitate upon the Water in the lesser Legg, so as to put it into motion, or expel it out of the lesser Orifice, for there is no Gravitation upon the Water in the lesser Legg, but by a kind of Thred or Line of Water, commensurate in Amplitude to the smallest Orifice of the *Siphon*, which being in *Equilibrio*,

4 Observations touching the

*librio*, at a Superficies of equal height, the Water in the lesser end of the *Siphon*, is not driven out by the over-weight or excess of Water in the greater Legg, for that excess is supported by the sides of the *Siphon*, and preseth not upon the lesser Orifice.

But if the smaller Legg were the longer, and filled with Water, while the other end is stopp'd, and then the greater Legg were opened, the Water would descend in the lesser Legg, and drive out the Water in the greater, till the Water in the lesser Legg had sunk to the same level with that in the greater; for regularly, the excess or advantage of Gravitation, is according to the length, not according to the breadth of the Column of Water, impending or pressing upon the Orifice, either in a straight or crooked Line.

But these kind of *Siphons* are not those touching which this Chapter is principally directed, but the erect *siphons* where the Leggs stand downward.

Second.

Secondly, Therefore touching these *siphons*, I shall subjoin some obvious plain Experiments or Observations, which yet in the conclusion, will lead us to the discovery of some things more abstruse and observable.

These *siphons* may be of several structures; some, where the Leggs are of equal length; some, where one Legg is longer than the other; some, where the one Legg is of equal or greater length, but yet one Legg is of a larger capacity than the other, or of an equal capacity, yet their Orifices are different.

Again, there may be an equality in the length of the Leggs, but a disparity by their several immersion in the Vessel of Water, upon which they exert their motions. There will be use of these various Instances.

First, if there be a *siphon* of unequal Leggs, suppose the Legg *A*, three foot long, and the *B* four foot long, invert it, and fill it with Water, and stop both Orifices with the Finger, and then turn the shorter Legg into a Vessel of

6      Observations touching the

Water, and leave the longer Legg in the open Air, and then unstop both Leggs, the Water will run out of the longer Legg, till it have emptied the Vessel of Water, wherein the shorter Legis immersed as low as its immersion.

And the reason is, because the Water in the Vessel and in the *Siphon*, are in continuity, and the two Leggs of the *Siphon* are like to a *Vectis*, or pair of Scales, and imitates the Laws of the *Libra*, or *Vectis* in many respects, and because the Water in the longer Legg is more weighty than that which is in the shorer Legg, above the Superficies of the Water in the Vessel, upon which the attraction is wrought, therefore the Water is drawn out, and becomes sequacious of the continued Water in the *Siphon*, and follows the traction of the longer column of water which is in the longer Legg. And the same reason, with some small variation, is in the traction of Water by filtration.

But if in the upper part of the *siphon* there be the least small Pin-hole, whereby the Air may be intromitted and at-

attracted into the *Siphon*, the raising and attraction of the Water in the shorter Legg will be impeded, for now there is an Element of a more subtil nature, and with more facility attractable by the descent of the Column of Water in the longer Legg, which with more ease performs the continuation of the parts of the Universe, than by raising the Water contrary to its own natural propensity: For though the overcharge of the weight of Water in the longer Legg, like the excess of weight in one of the Scales of a Ballance, is the immediate cause of raising the Water in the shorter Legg, yet the remote or rather (as I may call it) the final cause thereof, is the implanted Institution of the law of Nature, for the preservation of the unity of the Universe, and continuing of its parts without any *chasm* or interposition of mere empty space.

Secondly, The greater the excess of the external, or descending Legg is in proportion to the inward shorter, or ascending Legg, immerſed in the Water,

8 Observations touching the  
ter, the greater quantity of Water is  
vented by the longer Legg, answerable  
to that disproportion, so that more Wa-  
ter is attracted and vented in the same  
portion of time, where the external  
Legg exceeds the internal, by four foot,  
than where it exceeds it by two foot.  
And this is obvious to any mans Eye,  
for if the Leggs be equal, suppose  
three foot each of them, and the Legg  
A be immersed two foot in a Vessel of  
Water, whereby, as shall be shwon,  
there is in truth but one foot above the  
Water, the Water will spend from the  
longer Legg much faster than when the  
Water is sunk a foot lower in the  
Vessel.

And herein also is a resemblance be-  
tween the *Siphon* and the *libra* or *vectis*,  
but with this difference.

That if in a Scale; or *libra*, or *vectis*,  
the weight of a pound be appended at  
five foot distance from the *Hypomoclian*,  
it will counterpoise four pound at one  
foot, distant at the other side of the  
beam, from the *Hypomoclian* (weight and  
distance, answering each other.) But in

in the *siphon*, if the length of the external Legg of the *siphon* be four times more than the length of the shorter Legg, it will not draw out four times more quantity of Water than it would upon any small excess, of an inch or so, but only double the quantity: This we learn from the Experience and Observation of the Judicious *Mersen-nus*, in his *Phenomena Pneumatica*, prop. 35. *Docet experientia, &c. Esse semper crurum longitudines in ratione duplicata quantitatum aqua fusarum.*

And here I cannot omit, that Water is a consistence of as little connection and texture as *Lineus* his *Funiculus ac-rens vel effluvialis*. And yet, by the efficacy of the laws of Nature, and in obsequiousness to it, it is effective of Traction, in the instance in hand, as well as of Trufion, in some other cases. And this Traction is intended and improved in strength gradually, according to the length of the external or descending Legg: And yet perchance without due advertency, some would be apt to think it might deserve also

10 Observations touching the  
also the pleasant sensure of a Rope of  
Sand.

Thirdly, if the Orifice of the shorter Legg be for the purpose of the Diameter of the eighth part of an Inch, and the rest of the cavity and outward Orifice, be of half an Inch Diameter, the *Siphon* filled as before, and the shorter Legg immersed into a Vessel of Water, as before is shewed, the evacuation at the longer Legg, will not be in proportion to the greater, but the lesser Orifice; and although the whole *Siphon* will remain still full, yet it will discharge a thred of Water commensurate to the lesser Orifice, *viz.* of the shorter Legg, immersed in the Water: For the evacuation cannot be faster than the supply, which being but of the eighth part of an Inch, the *Siphon* cannot discharge a Column of Water of half an Inch Diameter, and yet continue its motion.

But it is true, the excess of the Amplitude of the external or descending Legg, may possibly be so much, and the weight of the bulk of Water so great,

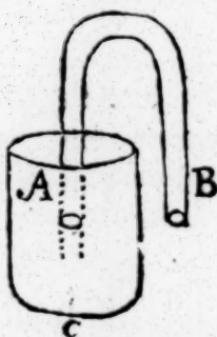
great, that it may disorder the motion, by descending down in a great mass, and then the Air will rise up through as it descends, whereby the Water in the shorter Legg will relapse into the Vessel.

Fourthly, Again, suppose the shorter Legg immersed in the Water, be of two Inches Diamater, and one foot long, and the outward discending Legg that dischargeth it self into the open Air be of two foot long, and but half an Inch Diameter, the whole Tube being filled and stopt, and the shorter Legg inverted into the Water, and then unstopt, the *Siphon* will run and discharge the Water out of the Vessel, till it come to the bottom of the short Legg, and yet the entire *Siphon* will still remain full; and yet the Water in the shorter Legg is above six times the weight and quantity of that in the longer Legg.

And the reason is, because the Gravitation of Water in motion is *secundum longitudinem Cilindri aquatici*, and not *secundum amplitudinem*, and the smaller

12 Observations touching the  
smaller Cilinder in the longer Legg  
makes his attraction, because longer  
than that in the shorter Legg.

Fifty, If the Leggs of the Tube



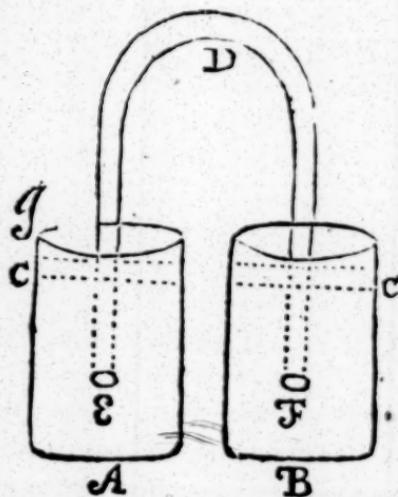
*A* and *B*, are of equal  
three foot length, and  
they are filled with  
Water stopt, and *A*  
Immersed into the Vessel *C*, of Water, suppose  
two foot deep in Water,  
and then both ends un-  
stopt, the Legg *B* shall  
attract the Water as strongly, as if it  
were really two foot longer than *A*.

And the reason is, because the Water  
in the Legg *A*, riseth two foot, not  
by any traction of the outward Legg  
of the *siphon*, but of its own strength  
in conformity to the height of the Su-  
perficies of the Water in the Vessel, so  
that only the foot of Water in the *si-  
phon*, above the Superficies of the Wa-  
ter is attracted and drawn up, by the  
force of the over-weight of the Wa-  
ter in the outward or descending Legg.

I therefore took a *Siphon* of some-  
what

what less than a quarter of an Inch Diameter in the Cavity, with equal leggs of two foot three inches long, and first filling the *Siphon*, and stopping both ends till one legg was immersed into a glass Vessel of seventeen inches of Water, the other legg hanging out in the Air, I observed, by the help of a Minute Clock, and a Scale of Inches applied to the side of the Vessel, the degrees of the depletion of the Cilindrical Glass Vessel; in the first Minute, the Water subsided three Inches and an half; in the second Minute, three Inches; in the third Minute, two Inches and an half; in the fourth Minute, two Inches; in the fifth Minute, one Inch  $\frac{1}{4}$ ; in the sixth Minute, one Inch and an half; in the seventh Minute, one Inch  $\frac{1}{4}$ ; in the eighth Minute one Inch; because, by every subsiding of the Water, the Cilinder of Water in the inward legg of the *Siphon*, above the Superficies of the Water in the Vessel, grew longer, and so had a greater counterpoise to the external Water in the outward legg, the Vessel of

14 Observations touching the  
of Water was Cilindrical of about  
six Inches Diameter.



Sixthly, if there be two Vessels of Water, *A* and *B*, of an equal or unequal bigness, but of an equal height in the Superficies of the Water in each Vessel at *CC*, and a *Siphon* of equal or unequal leggs, in length suppose of three foot long, *viz.* *D*, whose one legg is *E*, and the other legg *F*, and filling the *siphon* with Water, stopping each Orifice with the finger, till both leggs are immersed a foot deep in the Water, *viz.* the legg *E* in the Vessel *A*, and the legg

legg *F* in the Vessel *B*; now the Water in the *Siphon* will stand unmoved, because an equal length of a watry Column is in each legg, so that it stands in *Equilibrio*.

But poure more Water into the Vessel *A*, whereby it is raised three Inches, or more, or less, above the Superficies of the Water in the Vessel *B*, now the *siphon* will carry off the Water out of the Vessel *A*, till both Waters in each Vessel attain the same height of the Superficies, the like will be done if the Water be raised in the Vessel *B*, the Water will move in the *siphon* into *A*, till they come to the same height of Superficies.

And the reason is, because by raising the Water in either Vessel (suppose it *A*,) the attracting Column of Water in the other legg grows longer, and over-weighs the other, and so puts it into motion, as in the Figure.

And the like will be done, by taking out part of the Water in one of the Vessels, whereby the Superficies of the Water therein, sinks below the Super-

16 Observations touching the

Superficies of the Water in the other Vessel, the Water that was before in *Equilibrio*, and moved not, will now rise out of the fuller Vessel, and discharge it self into the lower, till the Superficies of the water in each Vessel be of equal height. And this, notwithstanding the equality of the length of both leggs, for the reason above given, *Nº 5.* for effectively, that legg is the longer, which contains the greater length of water above the common Superficies of the water, and therefore attracts upon the water in the other legg.

Now as touching the reasons of these motions, I have with as much clearness as I can, rendred those that I conceive the true reasons of them, which I shall not again repeat.

Others have thought of other reasons in favour of certain *Hypotheses*, that they have invented or entertained, such is that of *Des Cartes*, and of *Regius* a certain supposed circulation of all fluid Bodies, contiguous each to other, by a perpetual flux or succession, which though

though the thing be true, yet it is not an effectual reason of the *phenomenon*, in respect of the great disparity of the consistence of Fluids, in many instances.

But that which hath obtained with greatest applause, is the supposition of the Gravitation of the Air upon the Water, whereby the Water is impelled up into the *Siphon*, to any height, under 34 or 35 foot.

For it is supposed, that the ambient Air presseth round about the Legg of the *Siphon*, but it cannot press upon the Cavity of the *Siphon*, immersed in the Water, for that portion of Water subjected to that Cavity, is protected from that pressure, and so a pillar of Water, commensurate in weight to the Weight of the Atmospheric or airy Pillar is driven and kept up by that airy Pillar, into the ascending Legg of the *Siphon*.

And because a Cylinder of Mercury, of 29, or 30 Inches high, is equal in weight to a Cylinder of Water of the like amplitude, and 35 foot high, and

C both

both are equal in weight to a Cylinder of Air, or Atmosphere, reaching from the Earth as high as the Gravitating Air; therefore in all these Engins, the *Torriceilian* Engin, this of the *siphon*, and those of common Pumps, the Water is raised to 34, or 35 foot, and no more.

And therefore this of the *siphon* is not only brought as an Explication of that Hypothelis, but as a great argument to evince and prove the truth thereof.

But with their favour, I think the Argument returns to the disadvantage of this new Supposition; and that it is not possible for the supposed Gravitation of the Atmosphere to contribute one grain to the motion of the Water in the *siphon*: But the same is performed purely by the over-weight of the Water in the descending Legg, and this in order and conformity to that great instituted Law of Nature, to preserve the unity of the Universe, and the connexion of the integrals thereof. And this for these plain reasons.

First

First it is asserted by the whole System of this modern Philosophy, that their supposed Gravitation of the Atmosphere, though in various seasons or places it may have some little difference in its Gravitation; yet regularly it is uniform, and therefore there is rarely a variance above two Inches in the standard of the Mercurial Column in the *Torricellian Engin*, viz. between twenty eight and thirty Inches.

The consequence whereof will be, that the elevation of the Water in the *siphon* must needs be very near uniform and equal, and so must its evacuation consequently be, for if it be raised by that pressure, it cannot be raised more or less than what is commensurate to that pressure, nor with a greater swiftness than that pressure yields; for it is generally true, that where the motion of Water is swifter, the quantity of Water evacuated in the same time through the like Orifice must be greater.

But it is plain, even to the sight and view, that take two Vessels of the same

20 Observations touching the

Water, of the same content and superficies, in the very same place, at the same time; let a *siphon*, whose shorter immersed Legg is one foot, and longer outward Legg two foot, be set in motion in one Vessel, and another *Siphon* whose shorter immersed Legg is likewise one foot, and longer outward Legg is four foot, both *siphons* of the same bore or Diameter, the longer *siphon* shall carry off near twice the Water in the same time that the shorter *siphon* shall do it, so that it is the weight of the Water attracting, not the supposed gravity of the Column of Air, impending, that makes the odds; for the pressure of that must needs be uniform in both: For this see the second instance above given.

Secondly, take the sixth instance, where the ends of the *siphon* are immersed in two Vessels of Water, of equal Superficies, the Water in the *siphon* stands in *equilibrio*, and unmoved; take some Water out of the Vessel *B*, whereby it sinks an inch, or two, or three in that, or pour in one, two, or more

more inches of Water, into the Vessel *A*, whereby it is raised; now the Water will move out of the Vessel *A*, into the Vessel *B*, till it come to an equality of Superficies; shall we say, that the Column of Air, impending upon the Vessel *A*, drives up the Water into the *Siphon*? That is impossible; for in both Leggs, the Water subjacent to the Orifice of each Legg, are equally protected from the impending Column of Air, and the Column of Air impendent upon the Vessel *A*, that must give the motion, is in truth, shorter by three inches, than that impending upon the Vessel *B*, whose superficies is three inches lower than that of *A*, which, though it be but an inconsiderable thing in it self, yet it is sufficient to say, that therefore it cannot press more than the longer Column that impends upon *B*.

But shall we say, it is the advantage of the three inches of Water that is now given to the Vessel *A*? But neither can that effect it, for the additional Water in the Vessel *A* sits quiet,

22 Observations touching the

will not rise one point above its Superficies that it obtaineth, without a force upon it, to raise it above its station, its motion is never above its common Superficies.

But shall we say, that though neither singly can raise it in the shorter Pipe, yet *juncta juvant*? Neither will that all help the business, for the Airy Column upon the Vessel *A*, is overpoised by the aerial Column, impending upon the Vessel *B*, and therefore effects nothing, and the three inches of Water in *A*, neither of it self, nor by the aid of a counterpoised Column of Air, can never clime up by the top of the *siphon*, which is supposed at least a foot above it.

It remains therefore, that neither the Column of Air, nor the three inches advance of Water, nor both together, do not contribute one grain to this motion of the Water; but the same is effected by the traction of the longer Column of Water in the other Legg of the *siphon*.

Touching the height, whereunto  
Water

Water may be drawn by a *siphon*, there is great diversity of opinions.

The learned Author of *Hydrostatical Paradoxes*, pag. 179, tells us, it will not rise above 34, or 35 foot, which gives an *Equipondium* to the supposed *Column of Air* commensurate to its amplitude: On the other side, *Mersennus*, in his *Phenomena Pneumatica*, prop. 34. pag. 156, 147, tells us, *It may rise over the tops of Mountains.*

I cannot tell whether either of them ever experimented the thing; with Water it is hardly possible, considering the length of the *siphon* had need be 40 foot, and secure from any breach: And in Mercury, it is very difficult to keep that weighty fugitive Mineral from admitting some Air, as notwithstanding, several Essays I have found in a *siphon* of 4 foot long.

If, upon tryal it shall be found, that Mercury will rise in the shorter or ascending Legg of a *siphon* of three foot long, and will be discharged by the external or longer Legg of a *Siphon*, suppose four foot long, then Water will

24 Observations touching the

rise above forty foot in the shorter Legg of a *Siphon*, because it is generally admitted, that about thirty Inches of Mercury, the usual standard in the *Torricellian Engin*, countervails about thirty five foot of the like Cylinder of Water.

The consequence whereof will necessarily be, that neither the Mercury, nor Water, rising to that height, is impelled up by the pressure of the Air, because much exceeding the standard allowed, to make an *Equiponiam* between the Column of Mercury, or Water, and the imaginary Atmospherical Cylinder.

But if either the Mercury will not rise to thirty Inches, or not above, or the Water to thirty five foot, or not above, yet from thence no sufficient consequence can be drawn, that it is impelled up by the counterpoise of the Air, because other causes are assignable for it.

But in the mean time, 'till sufficient tryal be made ( to which I must subscribe ) I doubt, upon the reason of the things

thing, Mercury will not be drawn up to twenty nine Inches and an half, or thirty Inches, in the *Siphon*, nor Water to thirty five foot.

And this, not upon the account of the supposed *Equipondium* of the Air, to a pillar of Coal of that height; But for these two reasons.

1. Because, that although the over-weight of the Water in the external or descending Legg, hath connexion enough to raise a moderate weight of Water in the shorter Legg; yet it may be overcharged with the excess of the weight of thirty foot of Water, or more, and the like in proportion may be said for the Mercurial Cylinder in a *Siphon*, and therefore, in that respect it is true, the texture of the Water, though strong enough to raise a portion of Water, commensurate in weight to its *potentia*, will not be strong enough to raise a weight of Water, exceeding its *potentia*, no more than a Packthred will sustain as much as a Cable.

1. And therefore, it seems in this respect,

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spect, a *Siphon* hath not the same strength to raise so great a weight of Water, and discharge the same as a Pump, because the very Column of Water, in respect of its own weight may be less apt to hold its consistence than the aerial Column on the head of the Pump.

2. Because in the case of the excess of weight in the ascending Legg, being too heavy for the *funiculus aquaeus* to draw it after it; nature hath provided another expedient at hand, to preserve the connexion of the parts of the Univers, and to supply that which the strength of the descending Column in the longer Legg cannot effect, which is this; The *potentia* of the Column of Water in the external Legg, being over-charged with the weight in the other Legg, will discharge it self, and in the same moment, a Column of Air will rise up in the middle of the Legg, to the top of the *Siphon*, and by this means, the Water in the inward Legg will subside into the Vessel, and so by the intervention of the Air, the continuity

tinuity of the parts of the Universe is preserved, and all vacuities and interjected *hiatus* or *casmaes* of empty space is avoided.

And it is easie to observe this contrary motion of the Air ascending, and the Water descending, if a Tube of Water of about three foot long, and an inch Diameter, sealed at one end, stopt at the other with the finger, and their inverted, and then that end opened in the Air.

And this leads me to a farther discussion of the height whereunto Water may or may not be raised in a Pump, by exaltation.

Some learned and observing Men, tell us, that it will be raised to thirty five foot and no more; but others tell us, that it will rise forty foot; and some say, to any height whatsoever, *Vide Digbeian de Corporibus, pag. 160.*

I cannot Experimentally say, to what height it may be raised; But though I think it may rise higher than in a *Siphon*, yet I do not think it can be raised to so great an excess as 100, or 200 foot; and

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and the reason is, because such an excessive weight would over-match the strength and *potentia* of that *funiculus aereus*, that will attract and sustain a convenient weight of Water, suitable to its strength and continuity, which though it is difficult to determine the precise extent and limits thereof, yet we are sure is not infinite; the Nerves of my Arm, or a strong Packthred may lift up and sustain one hundred pounds weight, but not one thousand.

But then will not the Rope be broken, and a *vacuum* follow? certainly, nothing less, for the laws of Nature are so solicitous, as I may say, for the preservation of the integrity of the Universe, that in cases of extremity, or defect of one kind, it will supply it in another, when the Air is under the greatest tension, it can be in the Air Pump, or in any other, it will elude the strength of any man to raise the *manubrium* any longer, or if he can, yet the strength of that tension will percolate the Air, through the very consistence

consistent Water, in the Well, or if it cannot do that, yet it will separate the subtil Particles residing in the Water, or attenuate it into an airy consistence, which will supply the tensed Air, and give a relaxation to its tention, and that so effectually, that the Water in the Pump will thereupon subside. And this is obvious to any mans observation, by the Bubbles that are raised in the Air Pump, wherein Water is included, and many times, even in common Pumps; which Bubbles being full of an aerial substance, break, and so relieve the extream tention of the Air.

And therefore it is utterly inconsequent to say, that if the evitation of vacuity were the cause, why the Water riseth to 35 foot or thereabouts, it would upon the same account rise to 200 foot. For never any man yet pretended, that by all the strength that can be used, the Air can be wholly exhausted, no not out of that exquisite Vessel of the Air Pump, for when it is brought to the highest tention that

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can be by humane strength, yet the forrain Air will find some cranies or corner, to insinuate it self into it, and supply its tentions, or else, the tention of the Air will even relieve it self, by a supply which it borrows from the Water, as is before said.

And thus much concerning the *siphon* and its *phenomena*, by which it may appear, how ill the new Philosophy serves to the solution thereof, and how little reason there is to call them in to explicate, much less to prove the truth of this new Philosophy.

*Addenda to some of the precedent Chapters in this Book.*

**P**Ag. 13. It is there, and in some other parts of this Book said, That the higher position of Water in a Tube, above the Superficies of the Water in a Vessel, gives it an accidental Gravitation.

Two things are objected against this assertion: 1. That it is not found, for if there be a pair of Scales, if weight be appended at one end of the

Beam,

Beam, equal to that at the other end of the Beam, though the latter be appended with a farr longer string, yet both weights will hold an *Equipondium*.

2. That if the assertion were true, yet it is not safe, for it destroyes the whole design of the second Chapter, which denies a Gravitation of the upper parts of Water in a Vessel, so as to put it in motion. I answer,

1. As to the first, the instance given of the Scale is true, but yet it no way impugnes the truth of the assertion; For though in many things, the artificial Scale and the natural (such as is that of Fluids in some instances) do agree; yet herein the instance is misapplied.

1. If even a solid weight be in motion (as Water is in the case in hand) yet it gains an accidental Gravitation, by how much the more space it hath to descend, let fall the same weight from the Beam into the more distant Scale, and into the less distant, the percussion will be the stronger in that, than in this, which percussion is an accidental weight

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weight or Gravitation, and the case in question is concerning Gravitation of Water in the open Tube in motion not in rest.

2. Again in Water, any man that is pleased to consider it, will find, that if a narrow Tube of twelve inche high, hold a pound of Water, and a broader, but shorter Tube hold the like quantity and weight; the motion is swifter through an equal Orifice in the longer than in the shorter; for it *Gravitates secundum lineam longitudinis* not *secundum amplitudinem*, though the weights in themselves be equal *und Mersenn. in phenomenis hydraulicis prop. 9.*

3. Again, since the Water in the Vessel is of the same consistence with that in the Tube, there is no reason why the Water should not be sustained in the Tube, by the *basis* of Water in the Vessel, were it not for that accidental Gravitation that it obtains by its higher position.

2. And as it is not unsound, so certainly it is not unsafe, in relation to

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the second Chapter of the *Observations*, which contends, that in a closed Vessel, which hath no *Lumen* or Orifice to discharge it self, the upper part of the Water, though it increase the weight of the Whole, doth not Gravitate upon the lower parts, so as to put them into motion, for there the Vessel is one and the same, is closed below the Superficies of the Water the same, no aperture to give it vent into the Air, or another Vessel of Water. But now suppose there were an Orifice in this Vessel, whereby it may discharge it self into the Air, or into a lower Vessel of Water under it; now the Water in that Vessel will Gravitate upon that *Lumen* or Orifice, and that so much the more, by how much the Superficies of the Water is higher than the Orifice or *Lumen*: And the case of the Tube of Water, incumbent upon a Superficies of Water in another Vessel, is the same with the latter instance: For first, the Tube is open at both ends. Secondly, the Superficies of the Water in the Vessel is of a greater Amplitude

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34 Observations touching the  
than the Tube into which the Water in  
the Tube hath now a free liberty to dis-  
charge it self; therefore this instance  
doth not at all impeach the supposition  
of the second Chapter, for they are of  
different natures, and stand upon diffe-  
rent reasons.

If there be a Vessel of Water, and  
an Orifice made in the side or bottom,  
the inception of the motion of the  
Water is of that next the Orifice,  
which finding a passage open into the  
Air, presently presseth upon it, so that  
in order of Nature, this is the incep-  
tion of the motion, but the motion  
being *quid Continuum*, even at the same  
time, the Water next the *Lumen* pres-  
seth outward, a Column of sequacious  
Water follows it, and increaseth the  
swiftness of the motion, by how much  
the longer and taller that Column of  
Water is that impendeth upon the  
Orifice: But this no way concerns  
Water in a closed Vessel, which hath  
no aperture to press upon, and con-  
sequently, is at rest, and without any  
actual motion.

Pag. 43. An instance is given of a Porringer of five inches Diameter, filled with Shot, and weighing in the Air 78 Ounces, and losing of that weight in Water about nine Ounces; And holding the same weight upon the Scale, in a deeper or shallower immersion, insomuch that although it be immersed forty Inches in Water, it weighs no more upon the Scale, than when immersed but a quarter of an Inch below the Superficies of the Water, yet the Column of Water impending upon, and in Base, commensurate to the Porringer at forty Inches deep, is near a thousand square Inches of Water, and weighs near forty pound.

And although possibly at a vast immersion in depth of Water, suppose twenty or thirty Fathoms, there may be some small accession of weight from the impending Water, yet it will be utterly inconsiderable, in relation to the vast quantity of Water contained in a Column commensurate in Base to the immersed Body; for if it should,

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all the lines of Marriners Plummets would be broken at forty Fathom of water, and les, when yet they tell us a line of three pound weight will not be broken by an immersion of a *Bolis* of twelve pound weight, at two hundred Fathom depth of water.

This instance is applyed to two purposes. 1. That here the Column of water doth not Gravitate upon the Porringer at a more deeper than at a shallower immersion; for if it did, it must at forty Inches have an additional weight of forty pounds of water, depressing it, and must upon the Scale Gravitate accordingly; therefore solid Bodies as divers, &c. are not really press'd with any considerable weight of the incumbent Column of water. 2. That much less doth any Column of Air Gravitate in any considerable proportion upon the Mercury, or any other Body.

It is there also said, that upon the firſt immersion of the Porringer, under the Superficies of the water, there is a quantity of water commensurate

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to the bulk of the Porringer impelled up to the Superficies of the Water: But at all lower immersions, no greater proportion of Water is impelled up to the Superficies, but what was done upon the first immersion.

And this is certainly true, for in all the subsequent immersions, the Porringer leaving the place it had, gives way to the adjoyning Water to take up that room, without impelling any greater quantity to the Superficies than what was so driven up at the first immersion, *viz.* a quantity commensurate to the Moles of the Porringer.

The truth of the Experiment cannot be denied, only there is endeavoured to give a reason thereof, by the counter-pressure of a greater quantity of lateral Water, at a greater depth.

I shall not in this place dispute the reason, because done else-where, where I have considered the like reason assigned, touching the Non-pressure of the Air, therefore be the reason assigned true or false, yet the Fact it self being granted, it is most evident, that

the consequences are necessary, *viz.*

1. That the Column of Water doth not sensibly Gravitate upon the Porringer; for if it did, then of necessity the Porringer must weigh much more upon the Scale, at a deeper, than at a shallower immersion, and that considerably and sensibly.

2. That certainly, if the Fluid Water doth not sensibly Gravitate in any proportion, answerable to the weight of the Column of Water upon the Porringer, much less, can there be that incredible Gravitation assigned to a Column of Air, which is a more light and volatile Body, that hath a motion upward, as well as downward, and that hath a greater implication and contignation of its Particles, than Water hath; so that nothing is gained to the confirmation of the New Hypothesis, by the reason assigned, as long as the Fact it self is unquestionable, and the consequences of it little less than demonstrative.

*Pag. 227. 249.* I borrow *Linus* his expression of *Funiculus*, &c. and I think the

the expression significant enough to express the things intended: My Lord Bacon expresseth much the same thing, in his *Historia densi & rari*, pag. 73. by that which he stiles *motus nexus*, which possibly may please better.

But as to the thing it self, whereas it is said, that though as to trusion, Bodies that have not a connexion, or texture of parts, may be effective, yet as to traction, there must be a connexion and texture of the parts of the Instrument: And that Air or Mercurial effluxes, are not capable of such a texture, as will be accommodate to traction.

It is most certain, that by the institution of Nature, for the preservation of the continuity of the parts of the Universe, these Fluid Bodies have such a connexion, which though it may be not always proportionable to the strength of a Cable-Rope, yet is of sufficient strength to perform such a traction of such a *pondus*, as may be suitable in proportion, to its strength, and in a great measure, to the exigence

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of Nature, in such instances. And he that considers the attraction of the Filaments of the Air, in the *Magdeburgh* Hemispheres, discoursed at large, *Chap. 18*, and the attraction of the *siphon*, clearly evidenced in the Twentieth Chapter, will see no absurdity nor untruth in a traction, by a *Funiculus aereus*, or a *Funiculus equinus*, though not twisted into the consistence of a Cable.

As touching the equal subsiding of Liquors of several natures, as Wine and Water, unto thirty four, or thirty five foot, in a long Tube, closed at the upper end: This was experimented by Mr. *Pascall*, as it is related by *Shottus*, in his *Mirabalia varia*, pag. 100, and found true; But it makes nothing in favour of the *Equipondium* of the external Pillar of Air thereunto, nor against the solution offered above, *Chap. 15*. For if we give credit to the exact calculation of the disproportion of weighty Bodies, given by the Lord *Bacon*, in the beginning of his *Historia densi & rari*, A quantity of Water, suppose

pose less than an Inch square, weighs somewhat less than three Grains, and so doth the like quantity of Claret Wine, *viz.* two Grains *d. q.* the consequence whereof is, that their consistence is near the same, and an equal proportion of subtil parts, is in the one as the other; and consequently, the subtil parts of the one Liquor, as well as the other, abstractable by the same method, whereby to suspend the weight of thirty four, or thirty five Foot of either. And although the Spiritous or subtil parts of the Wine are more fiery than those of Water, yet their subtility are much of the same nature and measure, and consequently, by the same means, separable, to make up a *potentia sustinens*, of the one and the other.

*Pag. 176.* an instance is given of the covering of the *Torricellian* Engin, within a large Glass Receiver, whereby the supposed Gravitation of the impending Column of Air is intercepted, and yet the Mercury continuing still at twenty nine Inches high; from hence it is demonstratively concluded, that the

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the supposed Gravitation of the Air,  
contributes nothing to the suspending  
of the Column of Mercury.

To avoid this, there is substituted  
a supposed elasticity in the included Air,  
which performs the *Phenomenon*, and  
supplies what the Gravitation cannot  
in that instant effect.

And to prove this, it is asserted,  
that if the *Torriceilian Engin* be placed  
in the Glass Receiver, upon the Air  
Pump, by exhausting the Air, the  
Column of Mercury will considerably  
subside, according to the seventeenth  
of the *Phisico Mechanical Experiments*  
of the excellent person, formerly men-  
tioned. And it is said, that the rea-  
son hereof is the abating of the elas-  
ticity of the Air, included in the Recei-  
ver, by the extractions and rarefaction  
of that Air.

The Experiment it self is unquestion-  
ably true, but the reason, or cause  
thereof is ill assigned.

Against the imaginary elasticity of  
the common Air, enough, I think, hath  
been said in the twelfth and thirteenth

Chap-

Chapters, which I shall not repeate.

But as touching the evidence above given for that Elasticity, from the subsiding of the Mercury in the exhausting of the Air-Pump: It seems to me, a mere mistake, and an assignation of *Non Causa pro Causa*. And I need not say more therein than what *Linus* hath said, *Chap. 18.* upon that instance, which is but this, that by the strong Tention of the Air in the Receiver, after the exhaustion, the Tensed Air attracts upon the restagnant Mercury, and consequently, upon the Column thereof in the Tube, and draws it downward.

And that this is the true cause, and not the abatement of the pretended Elasticity, appears by the nineteenth, twentieth, and one and twentieth Experiments above mentioned, whereby it appears, that the Water in a Viol, placed in the Receiver, will after such Pumping, rise up and run over; and he that attentively considers this, must needs conclude, if he be not strongly pervidicated that this is effected by the strong

strong Tention of the Air, and attraction thereby wrought, upon the Water in the Vial, and not by any abatement of Elasticity of the Air in the Receiver.

If it be said, that the Mercury being 14000 times heavier than the Air, connot be raised, or drawn upon by the Tention of the Air.

I answer :

1. When two things are in *equilibrio*, though they may be of great weight, a small advantage given to one, will give it a preponderation. Let the two Scales of a Ballance be charged with an equal weight, suppose of forty pounds, a few Grains thrown into one Scale, will depress it, and raise the other as well, though perchance, not so much as if the Scales were empty, and the Grains cast into one of the Scales.

When the Mercury is suspended in the Tube, there is a kind of *Equipondium*, (not between the Mercury and the external Column of Air, as the new Philosophy would have it) but bewteen

between the *pondus depremens* of the Mercury, and the *potentia sustinens* of the included portion of Air, or subtil matter, in the top of the Tube, which by its Tention, and *motus nexus*, sustains it.

When the external Air in the Receiver is highly expanded, by the evacuation of the Air Pump, and is thereby put into a high degree of Tention, it endeavours its own contraction to its just size, as much as it can.

And in that *Conatus ad motum contractionis*, it layes hold upon the Bodies contiguous to it, by a *motus nexus*, and attracts upon them with the same strength as it endeavours its own contraction.

And by this means, the external Air on the Receiver, being by their Pumping brought into so high a degree of Tention, exerciseth as strong attraction upon the Body of the restagnant Mercury, as the internal Air exerciseth upon the Mercurial Column, whereby that *Equipondium* that was formerly, is abated, and the mercurial

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curial Column being in continuity  
with the restagnant Mercury, must ne-  
cessarily subside, by the raising of the  
restagnant Mercury, or any little in-  
clination thereunto, wrought by tra-  
ction of the now expanded and tensed  
external Air upon the restagnant Mer-  
cury.

2. Again, in case of an extream  
tention of the Air, its *motus contracti-  
onis* of it self, and consequently its  
*motus nexus* upon contiguous Bodies, is  
so strong and vigorous, that either the  
tention of the Air must be relaxed, and  
relieved by the admission of some com-  
mon Air into it, or else that *nexus*, with  
other contiguous Bodies, is not to be  
disjoined, without a great strength or  
weight to seperate and disjoin it, as ap-  
pears sufficiently in the Eighteenth  
Chapter, touching the *Magdeburgh* He-  
mispherie; and therefore it is no mar-  
vel, that the gentle Body of the Air,  
now put under so great a Tention,  
draws the restagnant Mercury upward,  
or at least works so effectually upon it,  
that it pulls down the Mercurial Co-  
lumn

lumn in the Tube below its common standard of twenty nine Inches and an half, and thereby necessarily stretcheth that little portion of included Air, or subtil matter, into a longer line or consistence than the bare weight of the mercurial Column would do by expansion.

3. Again, though the Tention of the Air will not raise up a Body of Mercury simply residing in a Vessel in respect of its close and weighty consistence, yet when part of it, namely, the Column in the Tube is pendulous, it is capable of an easier attraction by the Tension of the Air in the Receiver, which may be so vigorous and powerful, that it possibly may at least virtually and effectively pervade the Mercury even to the very impending Column and attract upon it.

And it must be remembred, that this attraction wrought by the Tention of the Air in the exhausted Receiver, is far more vigorous than the attraction wrought by the included portion of Air, or subtil matter in the head of the  
Tube,

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Tube, because under a strong tension.

These, or some of these considerations seem to be the cause of the subsiding of the Mercury, rather than the abatement of the supposed elasticity of the Air by its rarefaction.

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